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CONTENTS OF SERIES II, VOLUME 5

PAGE

Title page i

Contents iii

The Pleistocene Faunas of Asia and Their Relationships to Early Man. By Edwin H Colbert 1

The Action of the Heart and Circulation During Diving By Laurence Irving 11

The Problem of Fear By John Dollard 17

New Members 19, 42, 84, 128, 156, 185, 201

The Dating of Diastrophic Events in Southern Nevada. By Chester R. Longwell 21

Some Morphological Adaptations of Cetaceans (Whales and Porpoises) for Life in the Water By Henry C Raven 23

Primitive Law and Modern By E A Hoebel 30

Some Recent Studies on Photoperiodicity in Animals By Thomas Hume Bissonnette 43

Report of the Annual Meeting By Eunice Thomas Miner 52

New Methods in Stellar Dynamics By S Chandrasekhar (Abstract by Professor Jan Shilt) A Cressy Morrison Prize Winner, 1942 57

Comparative External Anatomy, Phylogeny, and a Classification of the Bees By Charles D Michener (Abstract) A Cressy Morrison Prize Winner, 1942 57

The Relation between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails By Arthur W and Priscilla F Pollister (Abstract) Honorable Mention, A Cressy Morrison Prize Competition, 1942 58

The Haplolepidae a New Family of Late Carboniferous Bony Fishes By T Stanley Westoll (Abstract) Honorable Mention A Cressy Morrison Prize Competition, 1942 60

Genetic Studies of Speciation in the Swordtail-Platyfish Group and of the Experimentally Produced Hybrids By Myron Gordon 63

Cultural and Thematic Analysis of Fictional Films By Gregory Bateson 72

On the Problems of German Characterology By Erich Fromm 79

Gravity Anomalies in Sedimentary Basins By Dorr C Skeels 87

Time Culture Method in Recent Investigations By Robert Chambers 92

Negro-White Interpersonal Relationships in a Limited Environment. By Theodora M Abel 97

Themes in Japanese Culture By Geoffrey Gorer 106

Conference on "High Polymers" 125

Conference on "Psychosomatic Disturbances in Relation to Personnel Selection" 126

Announcement, A Cressy Morrison Prize Contest, 1943 127

Ring-dikes and Their Origin. By Marland P Billings 131

Personality Development and the Social Psychology of Peace By Eugene Lerner 145

Food, Time, and Culture By G Evelyn Hutchinson 152

PAGE

Conference on "Parasitic Diseases and American Participation in the War" 155

Deep-focus Earthquakes. By WILLIAM A. LYNCH 159

Problems in the Behavior and Evolution of a Species of Blind Cave Fish. By CHARLES M. BREDER, JR. 168

Anthropological Approach to Dietary Problems. By MARGARET MEAD 177

Conference on "Sulfonamides" 183

Announcement of Publications for 1943 184

Mapping by Photogrammetric Methods. By GERALD FITZGERALD 187

Reconnaissance Mapping by Photogrammetric Methods. By GERALD FITZGERALD 187

Studies on the Chemistry of Chromatin. By A. E. MIRSKY AND A. W. POLLISTER 190

Some Aspects of the Problem of Unsuccessful Personality Screening. By DAVID SHAKOW 199

Conference on "Magneto Chemistry" 200

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SECTIONS OF GEOLOGY AND MINERALOGY

OCTOBER 5, 1942

DOCTOR EDWIN H. COLBERT, The American Museum of Natural History, New York City, N. Y.: *The Pleistocene Faunas of Asia and Their Relationships to Early Man.* (This lecture was illustrated by lantern slides.)

Within the past decade a great deal of attention has been devoted to the Pleistocene mammalian faunas of Asia, partly as a result of the discovery of the so-called Peking Man, *Sinanthropus pekinensis*, at Choukoutien, and, more recently, of the discovery of additional materials of the so-called Java Man, *Pithecanthropus erectus*, on the Island of Java. The separate discoveries of these extraordinarily important remains of ancient man in Asia were bound to stimulate all sorts of studies having to do with subjects related to early man in the Orient. Obviously one of the questions that loomed large in a consideration of the primitive human populations of Asia was that of the contemporaneous mammalian faunas. With what animals did early man in the East associate himself, and what is the significance of these mammalian assemblages with regard to the environment of the time? How may such mammalian faunas be utilized for dating early man in Asia?

Yet aside from the problem of early man, which is so interesting to us, Asia is none the less significant to the paleontologist interested in the later history of vertebrate life on the earth, for Asia occupies a central position among the continents of the world. It is a continental focal point, surrounded at greater and lesser distances by land masses to which it was, in Pleistocene times, readily accessible. This may readily be seen

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by a glance at a world map with the Pleistocene continental outlines restored. Here Asia appears, especially if a North Polar projection is used, near the center of the continental land masses, the eastern part of the great Eurasian continent. To the southwest is Africa, joined to the Eurasiatic continent by broad land connections. To the southeast is the East Indian region, not a series of islands as we know them, but a long peninsula, pointing its finger at Australia. And in the other direction, to the northeast, is the North American land mass, joined to Asia by a broad trans-Bering bridge, and in turn connected to South America via the still persistent Isthmus of Panama. Thus it is evident that Asia held a central position in the Pleistocene world of connected continents, a position from which active animals might push out in almost any direction, a position through which animals going from east to west and from west to east would have to pass. It is this great region, as it existed in Pleistocene times, with which we are concerned in the present contribution.

The study of Asiatic mammals of the Pleistocene may best be approached, however, by a subdivision of the general large Asiatic region into several distinct areas, dependent upon the location of the most prolific fossil-bearing localities, for the problem is too large and diffuse to be grasped by a single, undivided survey. Asia may be divided accordingly into five general areas, namely: the Indian region, in which are found the famous Siwalik faunas, the Narbada fauna and faunas of more recent age; the region of Burma, producing the Irrawaddy faunas and the cave assemblages of Mogok; the region of South China and Indo-China, in which are found terrace or floodplain deposits containing early Pleistocene mammals and later cave deposits extensively developed, such as the one at Yenchingkou in Szechwan Province; the region of the Malay Peninsula and the East Indies, notable because of the succession of faunas in Java; and finally, the region of North China with various faunas, of which the earlier Nihowan assemblage and the later Choukoutien fauna are of particular importance. (Incidentally it should be noted that of these five regions, the first four are included in the present-day Oriental zoogeographic realm, while the last one, namely the North Chinese region, falls within the present-day Palearctic zoogeographic realm.) It is in part by a comparison of the succession of faunas in these five regions that the Pleistocene history of Asia has today assumed the form of a logical and clear-cut story.

THE VILLAFRANCHIAN IN ASIA

One of the vexing problems that arises in any discussion of the Pleistocene in Asia (or in other parts of the world, for that matter) has to do with the delineation of the boundary between the Pliocene and Pleistocene periods. This is a problem of considerable importance because unless the limits of the Pleistocene period are definitely understood there will be confusion as to the entire larger problem of the Pleistocene faunas under consideration. In a measure there is a similar problem that applies to the boundary that may be drawn between Pleistocene and Recent times, but this problem is of less fundamental importance than the one dealing with the lower limit of the period, and need not be considered at this place. Furthermore, the subject of a Pliocene-Pleistocene boundary inevitably comes around to the much-discussed "Villafranchian problem," and it is only by a resolution of the Villafranchian problem in one direction or the other that a clear understanding may be had of what is intended when the word "Pleistocene" is mentioned.

The Villafranchian is named from the locality of Villa Francha d'Asti in Italy where there is found a very characteristic mammalian fauna. This Villafranchian fauna is notable for the first appearance in it of certain modernized mammals, notably the modern horse of the genus *Equus*, the first mammoths, *Archidiskodon*, and the first true cattle of the general *Bos* group. These are immigrant forms, the horse coming in from America, the mammoth, and probably the cattle also, from India or a closely adjacent region. Indeed, these Villafranchian elements, if so they may be called, appear almost simultaneously as migrants at widely separated regions in North America, Eurasia, the Orient and Africa, at a period just preceding the advance of the first continental glaciers from the north. Thus many paleontologists argue that the sudden "explosion" of certain modernized and widely ranging mammalian types from their centers of origin—the New World in the case of the horse and the camel (which reached many localities in Asia), the Old World in the case of the mammoth and the cattle—constitutes a valid and practicable datum to serve as a marker for the beginning of Pleistocene history. In accordance with this view the development of the Villafranchian faunas, a world-wide phenomenon, is regarded as the opening phase of the Pleistocene period.

ORIGIN AND SPREAD OF CERTAIN DIAGNOSTIC MAMMALS IN VILLAFRANCHIAN TIMES

	Africa	Europe	Asia	North America	South America
Horse	x←	x←	x←	origin	→x
Camel		x←	x←	origin	
Cattle	x←	x←	origin	→x	
Mammoths	x←	x←	origin	→x	

As against this view there is the school of thought that would regard the Villafranchian as marking the last expression of Pliocene history, a school which finds among its leaders the distinguished paleontologist, P. Teilhard de Chardin. Teilhard, as a result of his studies in Asia, has noted that the Villafranchian faunas of China, containing horse and mammoth, are on the whole of a subtropical nature, living, as is shown by the sedimentary evidence, at a time of extensive lake formation. The end of this Villafranchian lake stage in China, especially in North China, was marked by vigorous diastrophism during which the deep gorges of the Yangtze River were cut. After this period of gorge cutting there was a change in the physiographic expression of the land surface in China, accompanied by the advent of man in the primitive form of *Sinanthropus*. Upon the basis of this history Teilhard prefers to regard the Pleistocene in China, and in Asia generally, as beginning with the period of diastrophism and consequent gorge cutting subsequent to the development of the Villafranchian stage.

SUCCESSION OF PHYSIOGRAPHIC AND FAUNAL STAGES IN NORTH CHINA

Loess	Modernized man
Loam	First appearance of man
Gorge Cutting -	- - - - - - - - - -
Lake Deposits	Horse ← - - From Camel ← - N. America From - - - → Cattle India - - - → Mammoth + Subtropical Fauna

So it is that there are two sets of criteria for marking the beginning of the Pleistocene in Asia, and they may be represented in the following manner.

	Faunal Evidence	Physiographic Evidence
Pleistocene		
		Break Gorge Cutting
Villafranchian		
	Break Influx of advanced fauna	
Pliocene		

It is hereby proposed that the Villafranchian should be included within the Pleistocene period, since it is felt that the world-wide intercontinental migrations of certain modernized types of mammals such as the horse, camel, cattle and mammoth constitute a better datum for marking the beginning of Quaternary times than does the diastrophic movements which came at the end of the Villafranchian in Asia.

THE LOWER PLEISTOCENE FAUNAS OF ASIA

These are the faunas showing a characteristic Villafranchian stage of development, marked by the appearance of new and modernized types of mammals, some autochthonous, others immigrant forms. Among these Lower Pleistocene assemblages are the famous and rich Upper Siwaliks fauna of India, the Upper Irrawaddy fauna of Burma, the Djetis fauna of Java, the rather scanty Ma-Kai fauna of southern China and the Nihowan fauna of North China. That these faunas are closely related is shown by the identity or the close resemblances between certain important elements comprising each assemblage. Thus they may be compared in a general way as follows.

	India	Burma	E. Indies	S. China	N. China
Stegodont	x	x	x	x	
Mammoth	x	x	x		x
Rhinoceros	x	x	x	x	x
Horse....	x	x		x	x
Hippopotamus	x	x	x		
Camel...	x				x
Cattle...	x		x		
Orang.	x		x		

Certain of the differences between these faunas possibly are caused by the accidents of collecting; thus the fauna of South China is so scanty that it certainly does not represent a true picture of the mammalian life of that time in that region. On the other hand, some of the differences between the faunas probably reflect a real difference that existed at the time the animals were living. For instance, the lack of *Hippopotamus* in China very likely may be owing to the fact that these aquatic animals, seemingly of Indian origin, never reached the more eastern and northern portions of Asia. Likewise, the lack of camels in the East Indies may be attributed to the fact that these animals, coming into Asia from the northeast, never pushed into the then peninsular part of the Indies.

The noteworthy fact about this fauna is the absence of human elements in it. The higher primates appear in some regions in the form of orangs, but no human fossils are to be seen—an absence that may be accidental, but which on the other hand may be very real, reflecting the true conditions of the faunal composition at that time.

Conjectures as to the origin and direction of spread of the mammals comprising these Lower Pleistocene faunas of Asia are at best rather tenuous, but there is good reason to believe that the bulk of the mammals pushed out from an Indian center of origin—excluding of course certain immigrant forms such as the horse and the camel.

The Middle Pleistocene Faunas of Asia

These are the earliest post-Villafranchian faunas of Asia, which are set apart from the Lower Pleistocene mammalian assemblages by the very profound diastrophic break that so completely separates Villafranchian and post-Villafranchian history in this part of the world. Through China and into Burma these faunas are found in limestone caverns and fissures. In fact, the conditions of deposition and preservation of this "cave complex" are so similar in the various regions where they are found that, even without the evidence gained from the composition of the faunas, there can be but little doubt that we are looking at essentially a widely spread single assemblage of mammals. As cave fossils, there is a Middle Pleistocene fauna found at Mogok in Burma, Hoshangtung in Yunnan, and at various localities in southern China, notably Yenchingkou in Szechwan, the Kwangsi caves, and Tanyang cave near the mouth of the Yangtze. This southern cave fauna is related to the northern cave fauna of Choukoutien, and relationships may be traced between it and the stream-deposited faunas of the Narbada valley in India and the famous Trinil locality in Java.

	India	Burma	E. Indies	S China	N. China
Porcupine		x	x	x	
Bamboo rat		x	x	x	
Giant Panda		x		x	
Stegodont	x	x	x	x	
Mammoth	x	x	x	x	
Rhinoceros	x	x	x	x	x
Horse	x				x
Hippopotamus	x		x		
Camel					x
Cattle	x		x	x	
Orang			x	x	
Man (*Pithecanthropus*)			x		x

These Middle Pleistocene faunas, especially as they are developed in the caves of China and Burma, are characterized by the prominence of porcupine, panda, stegodonts, mammoths and rhinoceros as constituent elements. The faunas outside of the cave areas, particularly as developed in India and North China, show differences from the South Chinese and Burma assemblages that probably are not to be explained entirely by the accidents of collecting.

Naturally, *Hippopotamus* would not be expected in cave deposits developed in upland regions. Yet it is to be noted that the cave faunas, as well as the southeastern fauna of Java, are marked by the absence of horses and camels, although other ungulates occur in them, notably rhinoceros and certain types of cattle These are the local variations that are bound to occur when faunas are being compared over a wide area.

As in the Lower Pleistocene faunas, *Orang* is present in the East Indies, and at this stage it is especially prominent in the South China assemblages. The fact of prime importance in this connection, however, is the appearance of primitive man in two areas, namely Java and North China. This marks the first appearance of man in Asia, and he is in a notably primitive stage of development, namely the stage which has been designated as *Pithecanthropus* in the south and as *Sinanthropus* in the north. (Incidentally, in spite of the generic differentiation that has been accorded to the human remains of North China and of Java, they represent essentially a single type of man.)

Therefore the evidence of the mammalian faunas supports the view, first proposed some years ago by Dietrich and by other authors, that primitive man, as we know him, did not appear in Asia until Middle Pleistocene times, and supplants the earlier idea which accorded a Lower Pleistocene age to *Pithecanthropus*.

The directions of movement of the Middle Pleistocene faunas in Asia are perhaps even less clear than is the case for the Lower Pleistocene faunas. In fact, the evidence would seem to indicate simultaneous migrational flows in several directions: of Indian (Narbada) forms eastward into China, of Chinese forms westward into Burma, of northern forms southward into Java, and of Javanese forms northward into the continental area. The validity of such a picture can only be conjectured; at best it cannot be taken too literally.

THE UPPER PLEISTOCENE FAUNAS OF ASIA

Although a break between the Middle and Upper Pleistocene is to be discerned in Asia, it does not have the dimensions nor the significance of the post-Villafranchian interval of diastrophism. Consequently the Upper Pleistocene history of the region is more or less a continuation of its Middle Pleistocene history, a fact that is reflected to a large extent by the faunas. The Middle Pleistocene faunas continue into Upper Pleistocene times, with perhaps the disappearance of some older forms and the appearance of a few newer types to mark the change. Upper Pleistocene deposits are developed: such as the Potwar loess of India, the later terraces of the Irrawaddy River valley, the Ngandong deposits of Java, the Heichlungstun shelter deposits of southern China and the upper cave deposits at Choukoutien.

	India	Burma	E. Indies	S. China	N. China
Red dog or dhole			x		
Stegodont			x		
Mammoth . .		x	x		
Rhinoceros	x		x		x
Horse. . . .	x				x
Hippopotamus			x		
Camel. . . .					x
Cattle.	x	x	x		x
Man (*Homo*) .	x	x	x	x	x

The noteworthy fact shown by this list is the relative paucity of fossils in many of the Asiatic localities, a circumstance to be explained probably by the accidents of collecting rather than to any real diminution of the faunas in Upper Pleistocene times. As already remarked, the Upper Pleistocene was really a continuation to a large extent of the Middle Pleistocene in Asia, and in view of the many resemblances of

Middle Pleistocene mammals to Recent mammals of the Asiatic region there is reason to think that these animals continued with but minor extinctions into Upper Pleistocene times. The explanation is probably to be found in the fact that, in many localities such as those of India and North China, the deposits are aeolian loess, not conducive to the preservation of fossil faunas. In others, such as Burma, the deposits are such that fossils are very scanty, and when found are badly damaged by the action of water.

It will be noted that where the Upper Pleistocene faunas are well preserved, notably in the East Indies, they are composed in the main of a continuation of the Middle Pleistocene elements. The one very important and noteworthy addition to the Upper Pleistocene faunas of Asia is that of modern man, *Homo*. Man, in a paleolithic stage of development that is essentially neanderthaloid, as shown by the fine series of skulls discovered by von Koenigswald in the Ngandong deposits of Java, and by the flint industries discovered in the Soan valley of India and in the upper cave deposits and the loess of China, appeared in Asia to supplant his pithecanthropoid ancestor. Thus the stage was set for the later history of Asia, involving the changes in faunas and the shifting of human populations that led into Recent times.

THE TRANSITION INTO RECENT TIMES

It is difficult to draw a hard and fast line between the Pleistocene and the Recent, owing to the fact that, being so close to this transition, we can see those details which have been preserved to mark it as a gradual change. Various criteria have been adopted to mark the boundary between the Pleistocene and our modern age, just as various criteria have been used to designate the Pliocene-Pleistocene boundary. Perhaps for our purposes, the change from what we might call the Pleistocene faunas into Recent faunas can be used to mark this change.

Our Recent mammalian faunas are essentially impoverished Pleistocene faunas—impoverished by the rather sudden and inexplicable extinction of various large typical Pleistocene mammals over vast continental areas. Thus in Asia, the transition from the Pleistocene into the Recent, a transition that seemingly occurred only a few thousand years ago, was the period during which many large proboscideans, the stegodonts and the "mammoths" disappeared from the face of the land; when there was an extinction in the Asiatic region of the Pleistocene type of hyena (related to the modern African hyenas) and of the hippopotamus. Other mammals, still living in limited areas of Asia, would seem to have had their widespread ranges contracted during this transitional period, so that they were in effect extinguished over a greater portion of the Asiatic region. Such were the giant pandas, the tapirs and the rhinoceroses.

Finally, this transition was the time during which modern Asiatic man in a Neolithic stage of development pushed out in migrations and counter-migrations to set the pattern for the Recent distribution of man in the Orient. These movements were extremely complex and involved, and need not be discussed here. Suffice it to say that this was the time at which the mongoloids migrated to the east, through the Bering region, to cross from the Old World into the Western Hemisphere and thus populate a New World.

SUMMARY

The Pleistocene faunal history of Asia is divisible into two phases, sharply separated, each from the other, by a pronounced diastrophic break. The first or earlier phase is comparable to the Villafranchian of Eurasia and to the basal or Lower Pleistocene of North America, and is marked by the appearance of numerous advanced mammalian genera, some immigrants, other autochthonous. In this stage of the Pleistocene of Asia there is as yet no indication of the presence of primitive man.

The second or later phase of the Pleistocene faunal history of Asia is the post-Villafranchian stage, comprising Middle and Upper Pleistocene times. The Middle Pleistocene, coming immediately after the post-Villafranchian diastrophism in Asia, is characterized throughout China and Burma by the development of cave faunas showing close relationships to each other. It was at this time that man first appeared in Asia (at Trinil in Java and at Choukoutien in North China) in a pithecanthropoid stage of evolution. The Middle Pleistocene history of Asia passes without any very sharp breaks into Upper Pleistocene, with a continuation of the characteristic faunas, now developed in loess and terrace deposits. The most noteworthy change to be seen between these two ages is the disappearance of *Pithecanthropus* and the appearance of Neanderthal man, as shown by skulls found in the Ngandong horizon of Java, and by stone industries from India and China.

The transition from Pleistocene to Recent times in Asia is marked by the extinction of certain large animals from the Pleistocene faunas, and by the appearance of Neolithic man, who spread from his Asiatic homeland via a trans-Bering route into the New World.

SECTION OF BIOLOGY

OCTOBER 12, 1942

PROFESSOR LAURENCE IRVING, Edward Martin Biological Laboratory, Swarthmore College, Swarthmore, Pa.: *The Action of the Heart and Circulation During Diving.* (Illustrated by lantern slides.)

Diving seems, at first consideration, to be an unusual activity for a mammal because of the stress that diving places on a respiratory mechanism using air. But several orders, like the whales, seals and manatees, are thoroughly aquatic, and depend entirely upon diving to secure their food. A number of species from other orders, like the otters and beavers, live largely by means of their facility for feeding or working under water. These animals do not merely exist in a precarious state, but they flourish in large numbers, and form a considerable part of the total mammalian population, as can be seen by examining the records of the whale fisheries and fur trades.

From these recorded numbers, it appears that the divers successfully overcome any handicap imposed by diving habits which must place considerable stress upon a mammalian type of respiration dependent upon a good supply of fresh air. As a matter of fact, divers can easily suspend breathing for from 10 minutes to 1 hour. This ability to suspend breathing places the divers in sharp contrast with the non-divers that find difficulty in a minute or two of arrested breathing. The diving mammals are not of one related group, but they sometimes appear as single species, like the hippopotamus, among orders that are usually entirely terrestrial.

The common occurrence of diving species suggests the use for diving of faculties that are widespread among mammals. The use that the divers have made of their ability and the handicap, and even danger, imposed by the briefness of our ability to tolerate arrested breathing stimulate our interest in the respiratory reactions that are involved in diving. Their widespread occurrence indicates that we should look at a number of species, in order to compare their respiratory adjustments for diving and to see if they do not indicate the accentuation in the divers of common faculties of mammalian respiration.

The action of the heart and circulation can be especially well seen in the arrested breathing of divers. The earlier studies have already been reviewed (Irving, 1939), and references to them will be found in that review. Like many other early and significant observations on respiration, the slowing of the heart, or bradycardia, was first remarked upon by Paul Bert. In several other diving birds, the cormorant, goose and swan, bradycardia was observed by Noel Paton. It was first reported in mammals in the muskrat, by Koppanyi. I observed that when a beaver's head was pushed under water the heart could be felt to slow, and that, during the natural pauses in breathing of seals (*Phoca vitulina*), the heart usually slowed to half of its frequency while breathing.

Scholander (1940) showed by electrocardiograms that during experimental dives the hearts of seals (*Halichoerus* and *Cystophora*) slowed to one-fifth or even one-tenth of the rate while breathing. The change in heart rate was so rapid that the next beat after submersion might be delayed for 10 seconds.

In a series of studies marking a very pleasant collaboration, Dr. Scholander and I have continued together the investigations that we had begun separately. In the seals, *Phoca vitulina*, the bradycardia is as prompt and well sustained as in *Halichoerus* and *Cystophora* (Irving, Scholander and Grinnell, 1942a). Electrocardiograms of the hearts of porpoises (*Tursiops truncatus*) swimming in the open sea showed that they reacted to each dive by a bradycardia, but that in their short dives it was not as conspicuous as in the seal (Irving, Scholander and Grinnell, 1941a). By skillful management, Miss Henrietta Safford accomplished the difficult task of securing electrocardiograms from diving minks (*Mustela vison*), and found that the bradycardia was conspicuous even in their short dives. It would be interesting to examine the heart action of the mustelids and to compare the aquatic otter with the aquatic-terrestrial mink and terrestrial-arboreal martin.

The bradycardia of diving is not restricted to born divers, for it appears in the sloths, *Choloepus* and *Bradypus*, when their breathing is arrested (Irving, Scholander and Grinnell, 1942b). The armadillo, *Dasypus novemcinctus* (Scholander, Irving and Grinnell, unpublished), likewise shows the bradycardia. In fact, bradycardia would probably be observed in any mammal if the means of observation were properly arranged, for we have always observed it when the technique gave satisfactory records.

In skilled human divers, bradycardia occurs during diving, and we have recorded heart frequencies as slow as 30 per second in a man during vigorous swimming under water (Irving, Scholander and Grinnell, 1941a). The bradycardia can be observed most readily in men and women accustomed to swimming, but we are not convinced that the extent of bradycardia is proportional to the ability of the human diver, because a trained athlete is, for technical reasons, such a superior subject for observation. It is interesting that the easiest animal to study, man, was about the last in which it occurred to us to look for the bradycardia of diving.

The regular occurrence of bradycardia and the fact that it persisted during active swimming suggests that other vascular changes also take place during diving. During spontaneous pauses in the breathing of seals (Irving, Solandt, Solandt and Fisher, 1935) and beavers (Irving, 1937), as the heart slowed, the blood pressure either remained constant, or, sometimes, rose. The maintenance of blood pressure with decreased cardiac activity indicated that constriction of the peripheral circulation compensated for the diminished cardiac activity.

By following the changes in blood flow through tissues with a thermoelectric recorder for blood flow, it was shown that the flow through the muscles of a beaver decreased when the breathing was arrested (Irving, 1937). At the same time, the flow of blood through the brain increased, and this example of selective control of the peripheral circulation gave evidence that, during the diving bradycardia, the circulation was restricted in some tissues, but well sustained in the brain. Looking at the results of such vascular changes, it appeared likely that, during a dive, the decreased muscular blood flow would diminish the speed of exhaustion of the oxygen reserves, while the maintained cerebral flow would provide for the brain the first call upon the oxygen stores.

A series of subsequent studies showed that the selective regulation of the peripheral circulation during arrested breathing commonly occurred in dogs, cats and rabbits (Irving, 1938). The selective control of the circulation was not specific to beaver, nor characteristic of divers alone. The restriction of flow involved the liver as well as the muscles, and thus served to check a large part of the peripheral blood flow. The restriction of muscular flow was controlled by the sympathetic nerves, and, judging from its quickness and apparently purposive nature, it was very nicely regulated.

For technical reasons, it was more difficult to determine the condition of the peripheral circulation of the diving seal, and yet those animals seemed most likely to demonstrate the operation of an electively controlled circulation. During experimental dives of seals lasting 15 or 20 minutes, there was no increase of lactate in the arterial blood, but, immediately after the dive, lactate flooded the circulation (Scholander, 1940). During a long dive, large amounts of lactate were shown to accumulate in the muscles, so that the metabolic processes of the muscles were practically isolated from the circulation (Scholander, Irving and Grinnell, 1942). The steady formation of lactic acid in the muscles of rats during short dives had also been shown (Scholander, 1940). The deferred escape of lactic acid also appeared in the dives of the Florida manatee (*Trichechus latirostris*), (Scholander and Irving, 1941).

The isolation of the muscles from the circulation could be accomplished by constriction of the peripheral circulation. That constriction occurred was indicated by observations that Scholander (1940) made upon the lack of bleeding from vessels cut during dives. It was also suggested by the common retardation of the muscular circulation during arrested breathing, by the bradycardia, and by the sustained blood pressure during the bradycardia during spontaneous pauses in breathing (Irving, 1939). In the seal, during dives, the arterial blood pressure remained about normal, in spite of recorded heart frequencies one-tenth of those during breathing (Irving, Scholander and Grinnell, 1942a). Simultaneous records of changes in muscular blood flow and heart frequency in seals, as recorded by the thermoelectric blood-flow meter and electrocardiograph, showed that the flow through the muscles diminished immediately as the heart slowed down, and was promptly restored in recovery (Grinnell, Irving and Scholander, 1942). Even the transient fluctuations in heart rate were usually reflected by changes in muscular flow, and the flow record conformed with the view that compensatory peripheral circulatory changes accompanied changes in cardiac action.

It has been suggested that these changes in the circulation might be naturally integrated in a useful set of circulatory adjustments for the conservation of the oxygen stores during diving. A rather clear indication of this situation was provided by simultaneous records of heart changes and oxygen content of arterial blood of seals during diving. Two seals showed the bradycardia developing somewhat more slowly than usual. In those experiments, the observed rate of oxygen use from the blood was more rapid than in two other seals in which the heart slowed promptly (Irving, Scholander and Grinnell, 1941b). It was already known that the end of a seal's safe diving time coincided with reduction of the oxygen content of his blood to about 3 per cent by volume. In the two seals which promptly showed bradycardia, the rate of reduction of the blood extended the use of the oxygen stores over at least 15 minutes. With the slow development of bradycardia, the more rapid utilization of the oxygen stores led to dangerously low oxygen levels in less than 10 minutes.

It is not a new conclusion that changes in the heart indicate the sum of the changes in the peripheral circulation. But bradycardia is not commonly regarded as a usual mammalian reaction, because, for terrestrial mammals, the air supply for breathing is usually unlimited. For the divers, on the other hand, economy of breathing is essential, and it seems quite certain that the elective control of the circulation allows them to husband their stores of oxygen for selected distribution during a long dive. The sparing of oxygen use so accomplished can only defer the necessity for providing oxygen until recovery makes restitution, so that the net result of this adjustment is only a deferment of the breathing requirements, and not a net saving.

True economy in the use of oxygen appears, to some extent, in the depressed metabolism of the quiet seal during diving (Scholander, 1940) and in the even more conspicuous depression of the sloth's metabolism during arrested breathing (Irving, Scholander and Grinnell, 1942b). The three-toed sloth, *Bradypus*, can easily endure 20 minutes of arrested breathing. During this period the sloth's heart slows down, but the slowing is delayed and small compared with the bradycardia seen in seals. The rate of oxygen use from the sloth's blood is also quite rapid, so that within 8 minutes its oxygen reserves are depleted to a level that would be dangerous for a seal. The sloth, nevertheless, endures well for a further 10 minutes, with practically depleted oxygen stores. Sloths can really endure asphyxia, but seals are not at all resistant to asphyxia and owe their long dives to the nicely controlled use of their available resources.

Other mammals show, more or less well, both hardiness toward asphyxia and vascular adjustments toward arrested respiration. Man appears to have little endurance for asphyxia, and, as indicated by the slight human bradycardia in diving, can make only poor adjustments for sparing his oxygen stores. Men are poor divers in comparison with seals, but even their moderate ability is important when it is all that they have for emergency use when accident or sickness arrests breathing.

The circulatory adjustments of mammals to diving appear to have common characteristics, but the development of these characteristics varies widely in the different species. The variations on the common adjustments seem to give to each species the physiological distinction by means of which respiration provides for the activity of each special habit of life.

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JOINT MEETING

SECTIONS OF PSYCHOLOGY AND ANTHROPOLOGY

OCTOBER 26, 1942

DOCTOR JOHN DOLLARD, Institute of Human Relations, Yale University, New Haven, Conn.: *The Problem of Fear*. (This paper was prepared under the joint authorship of Doctors John Dollard and Neal E. Miller.)

The association of fear responses to danger cues seems to take place by the usual mechanisms of learning. Fear responses made in circumstances of battle are no exception to this rule. Those who know the dangers peculiar to battle conditions are afraid when they go into action. Fear is a drive that, like any other drive, can incite responses that prove adaptive. Fear can make the soldier cunning and properly cautious.

Attention should be concentrated on the fear cues until fear has done its maximum work. When nothing is to be gained by further response to fear stimuli, it seems to be adaptive to take the attention off the signals of fear. Any number of distraction techniques can be used, such as "having something else to do while waiting for attack," singing, playing cards, joking, and the like. When fear must be borne, as it must under battle circumstances, the important thing is to strengthen all drives that compete with fear. Patriotic feelings, loyalties to unit, devotion to comrades, worship of the commander are all secondary drives that can compete successfully against fear.

Opinions seem to vary as to whether men are more or less afraid with repeated exposures to danger. Some seem to be, other not. Fear, of course, is contagious. So, also, is courage. In actual battle situations it seems highly important for the soldier to suppress sounds or gestures indicating pain or fear. It is possible, though by no means certain, that the discussion of fear outside of battle situations is useful. Some say that it reduces the sense of isolation of the soldier. Others add that it increases the force of a good example to know that another man is afraid. Some men seem to fight better when they are afraid, if the perception of their personal danger is accompanied by a parallel perception of danger to the group or society to which they belong. It seems quite likely that unconscious fear is usually dangerous, because unlabeled fear cannot be opposed by thoughts that would tend to control it.

Because of the confidential nature of the research data on which this report is based, it is necessarily sketchy. Also, collection of data has just begun, so that the ultimate form of such a report cannot be formulated at this time.

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 2, 1942

PROFESSOR CHESTER R. LONGWELL, Yale University, New Haven, Conn. *The Dating of Diastrophic Events in Southern Nevada.* (This lecture was illustrated by lantern and kodachrome slides.)

The highly complex geologic structure of southern Nevada reflects several widely separated crustal disturbances. In the Muddy Mountain area the stratigraphic and structural records are more complete than in other parts of the region studied thus far. Paleozoic formations exposed in that area have a total thickness of about 8000 feet. Mesozoic deposits, including at least 2000 feet of Upper Cretaceous strata, are 8000 to 10,000 feet thick. Tertiary basin deposits have a maximum thickness of several thousand feet.

Several disconformities in the older part of the section suggest wide epeirogenic movements; the most conspicuous of these breaks are at the base of the Moenkopi formation (Lower Triassic) and below the distinctive Shinarump conglomerate (Upper Triassic). The earliest large-scale localized movement in the general region is recorded in coarse conglomerate that lies with slight angular unconformity on the Jurassic sandstone. This conglomerate, laid down by streams flowing eastward, is nowhere more than 20 feet thick; it is made up of rounded pebbles and boulders, up to a foot in diameter, derived from all older formations down to and including the Permian. Since the clays and sandstones above the conglomerate yield a flora and fauna of early Upper Cretaceous age, there was strong uplift west of the Muddy Mountains either in Lower Cretaceous or in late Jurassic time.

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The first great orogeny in the immediate region produced gigantic thrust faults and localized intense folding. Horizontal displacement on the Muddy Mountain thrust was at least 15 miles, in a west-east direction; Cambrian limestone overrode Jurassic sandstone. A large thrust at a higher structural level is well displayed in the northern part of the Muddy Mountain area. The thrust plate has been removed by erosion, except for scattered remnants, in an area 20 miles long and 3 miles wide. Formations directly below the thrust lie in great recumbent folds which are broken by steep reverse faults dipping both eastward and westward. Formations in the overturned limb of a recumbent syncline are exposed in several elliptical domes that have been breached by erosion. On the eastern flanks of these domes, the strata have been turned through more than 200° from their original attitudes.

Upper Cretaceous beds are involved in the thrusting in the northern Muddy Mountain area. It is believed that the orogenic movement began in late Cretaceous time, since sediments of that series grow steadily coarser upward in the section. Great fans containing blocks of limestone 10 to 20 feet in diameter extended eastward from the thrust front. Evidently there was subsidence in a large area, since these coarse fan deposits are 3000 to 4000 feet thick. Over-riding of the fan debris by the thrust mass is indicated by a fragment of the thrust plate, nearly half a mile long, embedded in the coarse fan deposits, now tilted and bevelled by erosion.

Several large normal faults came into existence in front of the thrust while the thrusting movement was in progress. Other steep faults formed much later, probably with movements at several dates during the Tertiary and continuing into Recent time. The basin-and-range structure resulting from these faults is superposed on the earlier thrust structure. While the great faults were active, the depressions between the present ranges received interior drainage, under an arid climate. Since the appearance of the Colorado River in the region, much of the basin fill has been removed by the river and its tributaries. As a result the structural relations are exceptionally well displayed in this part of the Basin-and-Range province.

Field work on which these results are based was supported jointly through several seasons by Yale University and the U. S. Geological Survey. The writer gratefully acknowledges also two grants from the Penrose Fund of the Geological Society of America, in 1934 and 194[illegible].

SECTION OF BIOLOGY

November 9, 1942

Henry C. Raven, Department of Comparative Anatomy, The American Museum of Natural History; *Some Morphological Adaptations of Cetaceans (Whales and Porpoises) for Life in the Water.** (This lecture was illustrated by lantern slides and motion pictures.)

Whales are completely adapted for life in the realm of the open sea; so much so that the difference between a typical land mammal and a whale is scarcely less than that between some reptiles and birds.

The origin of the cetaceans is partly shrouded in the rather indistinct palaeontologic record of the early Tertiary Period. Many important specimens representing morphological stages have nevertheless been recovered and these reveal more or less the course followed in the evolution of the modern Cetacea. Many palaeontologists have described the remains of fossil whales. Of the more recent authors, Dr. Remington Kellogg is the most outstanding worker in this field.

The anatomical organization of whales reveals throughout evidence of their having been derived from land mammals. By comparing land mammals and pinnipeds of today with cetaceans, we may get an inkling of some of the first and more superficial changes that took place in the ancestral whales. If we look at the pelage of a polar bear, we see that it is uneven, with short hair on the head, longer hair on the back and the longest hair on the under side of the body. In all the seals, however, which spend much of their time in the water, the hair is short and almost uniform over the whole body; in the cetaceans it is lacking entirely except for rudimentary vibrissae.

The skin itself also shows striking differences. In the bear it is loose and thin, in the seals it is firmer and thicker, and in cetaceans it is perfectly smooth and very thick. Furthermore, in the cetaceans the skin is without the temperature-regulating sudoriferous glands, but there is a very thick layer of fat or blubber in and beneath the skin to insulate the whale against the temperature of the water, which in the sea is usually much lower than that of the animal.

Walmsley (1938, p. 168) in discussing this matter says: "Whales live for the most part in the high seas, seldom passing into shoal water except in difficulties. They migrate each year from Arctic to tropical waters. The temperature of the Arctic waters is 4° C or less, and of the tropical waters, 25° C or more. The body temperature of the whale has been given as 31.1°, 36.6°, and 36.9° C, that is, a little lower than that of the horse (38.2°) and the sheep (40.2°). As there are no sweat glands in whales, the body temperature must be regulated by the altered activity of the blood vessels of the skin and or the control of the basal metabolic rate. The blubber, however, it is generally held, is a protection against the cold of the Arctic waters."

Now let us consider some of the aquatic adaptations of the cetacean respiratory tract. First is the position of the nasal opening or blowhole. In modern whales this may be single or double and is near the vertex of the head, not near the front of the mouth. The blowhole is formed by the outgrowth of the sides of the nose beyond the median nasal septum; the nasal septum is present of course in all Cetacea.

In the skulls of some of the Eocene fossil cetaceans the nasal opening was not nearly so far up toward the vertex as in modern whales, though even then its retreat from the tip of the snout was well under way. The mechanism for closing the nostrils was evidently well developed in Eocene whales. A mechanism for excluding water from the respiratory passages during submergence is one of the first requisites of aquatic mammals, as indicated by its presence in such varied animals as the beaver and otter; it is more highly specialized in the pinnipeds. Apparently the deeper these aquatic mammals dive, the tighter their nostrils are closed and the larger and stronger this nasal mechanism becomes.

Dissection has revealed that the mechanism for closing and opening the nostrils or blowhole of whales is composed only of structures common to all mammals. These are: the premaxillary, maxillary and nasal bones, the septal and alar cartilages and the nasal portion of the superficial facial musculature, which is attached to them and innervated by the seventh cranial nerve. In the toothed whales these structures have undergone remarkable changes in size, proportion and function.

The final result, though varying considerably in the different whales and porpoises, has the same function in all. It may be briefly described as follows. Compressor muscles of the nose surround a fibrous fat-filled sac called the melon, or spermaceti organ, the rear end of which is divided into two fibrous projections or plugs covered with smooth black epithelium. When the encompassing compressor muscles contract, exerting pressure on the spermaceti organ, the free projections are forced against the external bony narial apertures, effectually closing them; meanwhile the blowhole at the surface, partly by the action of its compressor muscles and partly by other muscles that arise at the lateral borders of the base of the rostrum and insert in the skin on each side of the blowhole, is pulled forward and outward (rostro-laterally) at the corners and is thus snapped shut. This arrangement not only affords a powerful means of closing the nose but an equally efficient one for converting it in a fraction of a second into a spacious tubular passage through which used air is quickly expired and fresh air inspired.

While considering the nose and before going on to the remainder of the respiratory tract, it might be well to refer to the olfactory region in whales. In the earliest vertebrates the sense of smell was well developed and continued to function as a distance receptor of odoriferous substances, in contrast to the sense of taste, which functions only on contact. In land-living vertebrates the olfactory apparatus was modified to function only when air passing through the nose came into contact with the moist olfactory end organs. Air passed through the nose not only when it was drawn into or exhaled from the lungs in breathing but also during the act known as "sniffing" or smelling, which consists of short and rapid inhalations and exhalations that allow air to be tested for scent in the nostrils.

When the ancestors of the whales ceased to go to land and spent much of their time submerged, with their nasal passages tightly closed, they had no use for a sense of smell. Further, it is evident from an examination of cetacean skulls that the olfactory parts were reduced and lost even in the fetal stages of odontocetes, and though retained were greatly reduced in the fetal mysticetes.

To proceed with the adaptations of the respiratory tract: some of the palato-pharyngeal muscles have been transformed into an extremely strong sphincter (Huber, 1934, p. 124) that functions to hold the elongated upper portion of the larynx, composed of the epiglottic and supra-arytenoid cartilages, in intimate relation with the internal nares. The spout-like projection of the larynx can be easily held by the sphincter because the upper extremity of the epiglottis has abrupt lateral expansions. These, the laryngeal cartilages and muscles, are described in detail by R. Anthony and F. Coupin (1930) for *Mesoplodon*. This arrangement would preclude the entrance of water into the respiratory tract via the mouth and pharynx.

The trachea is capacious and short, in keeping with the short neck in all Cetacea and the habit of very quick inspiration and expiration.

The diaphragm in Cetacea extends farther forward ventrally and farther backward dorsally than in land mammals, thus not confining the buoyant air to the forward part of the animal but allowing much of it to be disposed more caudally, so that the lungs occupy the dorsal part of the body cavity except in its most caudal part. This arrangement is doubtless analogous to the disposition of the air bladder in many fishes, which contributes to the balancing effect.

Whales are subjected to great pressure when they make deep dives and the sperm whale is known to go deeper than others. Kellogg (1940, p. 37) records the following incident:

"In April, 1932, the cable repair ship *All America*, while investigating the cause of an interruption in the service of the submarine cable between Balboa, Canal Zone, and Esmeraldas, Ecuador, hoisted to the surface a dead 45-foot Sperm Whale from a depth of 3,240 feet off the coast of Columbia.

"The cable was twisted around the lower jaw of the animal and an additional 180 feet was wrapped around one flipper, the body, and the caudal flukes.

"At such a depth the body of the whale must have been subjected to a pressure of approximately 1,400 pounds per square inch."

The pressure is exerted equally on the whole surface of the animal, and Walmsley (1938, p. 171) states that "The increase of pressure will diminish the volume of air in the lungs and, omitting changes in temperature, 100 volumes at one atmosphere pressure are reduced to 10 volumes at 11 atmospheres pressure; with a decrease in temperature there is a slightly greater reduction. This large decrease in the volume of the contained air must be accompanied by a considerable diminution of the volume of the lungs and this probably explains the large amount of elastic tissue in the thick interalveolar walls of the lungs of whales."

When the lungs are so reduced in volume, there is also a reduction in size of the whole thorax and of all the organs not contained in bony cavities. The abdominal organs are doubtless forced into the thorax to occupy part of the space filled by the lungs before they were compressed.

According to Walmsley, the retia mirabilia are gorged with oxygenated blood which has been forced from the lungs when the animal dives and is under great pressure. Thus the central nervous system is well supplied with blood while the animal is down.

The adaptations of the digestive tract of whales are numerous. In the more generalized of the modern toothed whales the teeth have lost their conspicuous primitive differentiation into incisors, canines, premolars and molars. Here polyisomerism, or de-differentiation has been at play, resulting in an increased number of uniform, peg-like teeth of no use for chewing but ideally suited for grasping slippery fish, which form the greater part of the diet of most odontocetes. In the large killer whale the teeth are of the same type but much more powerful and are not only used for grasping fish but in tearing the flesh of seals and even large baleen whales.

From the homodont type of dentition mentioned above there are numerous specializations, usually with a marked reduction in the number of teeth. In the white whale (*Delphinapterus*) they are reduced to comparatively few very small teeth in the fore part of the jaws. In the narwhal there are no teeth in the lower jaw and in the upper jaw two maxillary teeth have developed as tusks. In the female these usually grow to a length of eight or ten inches but remain unerupted and embedded in the maxillary bone throughout life. In the male, however, the left one becomes the great straight, spirally-twisted tusk, seven feet or more in length, for which the animal is famous. These tusks and the one or two pairs of mandibular teeth developed in ziphioid whales have become secondary sexual characters and probably are of no use in the prehension of food.

The tongue in odontocetes is long and extends at least to the proximal border of the mandibular symphysis. It is free only at its extremity, if at all.

In the mysticetes, or baleen whales, the food consists of myriads of small pelagic invertebrates and fishes not larger than sardines. This group takes its name from the frayed horny plates of baleen, which are a series of epidermal outgrowths on each side of the palate. The mouth in these forms is enormous. The very long, laterally convex, edentulous mandibular rami are surmounted by flanges of stiff, fibrous connective tissue, which form a wall outside the baleen with its upper border meeting the arched rostrum.

The animal feeds by moving forward with its mouth open. The water passes into the front of the mouth and out at the sides between the numerous baleen plates on which the food collects. When the mouth is closed, the tongue, which may weigh tons, is raised, squeezing out the excess water, after which the food is swallowed.

The fauces in the comparatively small odontocetes is large, so large that the killer whale has been known to swallow seals entire. In the mysticetes, however, the fauces and oesophagus are extremely small, so that only small objects may be swallowed.

The stomach is complex and unlike that of any other mammal. The first compartment, which is the largest, appears to be a dilatation of the oesophagus. It is lined with thick epithelium, which is folded and forms a series of ridges when empty, indicating that it is capable of considerable expansion. The second compartment is the true digestive part which contains the gastric glands. Then follow from one to five more compartments, corresponding to the pyloric portion of the ordinary mammalian stomach. With the exception of the first compartment, all are lined with smooth, soft, glandular mucosa.

The intestinal canal is extremely long and narrow; it is given by Flower (1872, p. 428) for *Tursiops* as fifteen times the length of the animal. In the odontocetes there is no caecum nor any outward distinction between intestine and colon, though the colon may easily be distinguished by its mucosa, which lacks the villi of the intestine.

The mysticetes differ from the odontocetes in having a caecum and the arrangement of the intestine and colon similar to those of land mammals, with ascending, transverse, and descending parts of the colon.

The liver is small and not much divided in whales, and there is no gall bladder.

In the reproductive system there are also many adaptations to life in the water. Chief among these in the male is the streamlining of the body by the incomplete descent of the testes. They descend to the body wall in some cases but there is never any external indication of them. The penis is conical and pointed, with no evident terminal expansion, and may be entirely retracted within a preputial sheath. The only remnant of the pelvis in odontocetes is the ischium, to which the crura of the penis are attached.

In the female the vagina is provided with a series of circular folds (Hein, 1914, p. 63), which during coitus doubtless function to expel or exclude water from this system in conjunction with the conical intromittent organ mentioned above.

At birth the young are very large and precocious. They must be able to swim beside the mother, to come to the surface to breathe and to be suckled under water.

The mammary glands, unlike those of land mammals that form bulges on the ventral contour of the animal, are very much flattened to conform to the general streamlined pattern. Furthermore they contain large milk sinuses. Covering these glands there is the very highly developed paniculus carnosus muscle, which forms a broad sheet an inch or more thick in a fourteen-foot narwhal. This muscle acts as a compressor of the mammary gland to squirt the milk when the baby grasps the teat with its mouth. The two teats are retracted into grooves except when the young is suckled, an arrangement somewhat analogous to the retractable landing gear of an airplane.

Many aquatic adaptations are present in the locomotor system, most of them in connection with streamlining, such as the complete loss of functional hind limbs, the use of the fore limbs only for balancing, and the exclusive use of the tail for propulsion.

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SECTION OF ANTHROPOLOGY

NOVEMBER 23, 1942

DOCTOR E. A. HOEBEL, New York University, New York, N. Y.: *Primitive Law and Modern.*

There are moments in field work that brighten the ofttimes unrelieved drudgery of patient dredging for facts—orderly minutia. Field work among American Indians, especially, has long since ceased to be a larking adventure in prospecting. Civilization and acculturation have washed over the Indians too much for that. The gold panning techniques of the past century have given way to a systematic hydraulic dredging, which sifts out the pay dust but only rarely excites the operator by turning up a pure nugget.

Yet, such moments that thrill and excite do occur. And one type of nugget which flashes to light, giving a sustaining lift to the hours of routine work, is the utterance of a pithy epigram from the mouth of an informant or interpreter—an utterance that is refined of the juices and materials which your interrogations have set to work. Your native has seen beyond your queries which at first seemed senseless to him; he has seen beyond the material fact and into the meaning of things; he has caught the purpose of your groping, and because he is not a professor or a social scientist, he has conceived his insight simply, and he forms his inspiration in a quietly uttered reflection that sums up in a single phrase what you must elaborate in a book, or at least a chapter.

One such bit of epigrammatic insight was dropped into my lap by High Forehead, my Cheyenne interpreter. It serves well as a motif quotation for any discussion of primitive law, I think.

In the investigation of Cheyenne law-ways High Forehead had been both intrigued and perplexed by the line of questioning I had been following. "Mr. Grinnell never used to ask questions like those," he commented again and again when we were alone in our rest periods. The questions bothered him because they were for the most part searching inquiries into the ways of conduct in conflict and dispute—rules of conduct embedded in the behavior and attitudes of the Cheyenne people in action in the days before the triumph of White dominance. High Forehead was bothered, because the line of questioning seemed to him to put emphasis on the bad side of Cheyenne behavior. He was bothered, too, because he had his own traditional conception of anthropological interests based on his long experience with Grinnell. Anthropologists ought to be concerned with the more tangible and showy aspects of culture: particularly, ceremony and ritual, hunt and war parties, food getting, bead working, and stories—not stories about the troubles and squabbles of real people, who must be named, but legends and myths about culture heroes. This is what bothered High Forehead. But gradually he began to perceive that what was done about trouble was more important to our interests than mere misbehavior. The realization slowly obtruded upon his consciousness that the Cheyennes, his people, had on their own responsibility in their time dealt with and met in their own way many problems of social order. And, one noon, as we lay beneath a cottonwood tree, beside a sluggish creek, under a blue Montana sky, he looked at me long, and quietly said, "The Indian on the prairie, before there was the White Man to put him in the guard house, had to have something to keep him from doing wrong."

Yes, the Indian on the prairie had a social order to maintain. And so has every other society of man. Every people has its system of social control, some effective and some less so. And all but a few of the poorest have as a part of their control system a complex of behavior patterns and institutional mechanisms that we may properly treat as law. When this area of behavior we speak of as law is found in the culture of a preliterate people, we call it primitive law. When it is found in the culture of contemporary or recnet literate people, we call it modern.

Tonight we are concerned with Primitive Law and Modern. Implicit in this title is the proposition that there are points of comparability in Primitive Law and Modern. Each is but a part of a common universe of human experience which has grown out of an as yet not completely recognized, and certainly not perfectly understood, set of recurring problems that all societies must face and solve, if they are to exist as successful groups. We recognize a society as a human group acting in accordance with its unique culture. It is an aggregation or population of human beings faced with the fact that it must maintain a successful group order, if it is to continue its social existence. The mechanisms of maintenance are, we know, determined by culture, which defines not only what men shall eat and how they shall eat it but also how they shall conduct themselves in relation to each other. Culture not only provides for nourishment of the body but also provides for the sustenance of society. As a part of the social order, which is a guarantee of perpetuation of the group, we have the legal order, which exists in Law with a capital L.

In speaking of Law with a capital L one infers directly that there are other orthographies of this mighty little three letter word which grows out of trouble, feeds on trouble, and is itself so very troublesome. There is, of course, Law and law, different in capitalization, different in tonality, and—different in content and meaning. And as for law with a small l, it has its own diverse meanings, determinable only from its context, if at all.

It is, we must complain, a lamentable defect of our language that we are lacking in distinctive and effective words for each of the several concepts covered by this one word, law. Our teacher, Professor Boas, has long championed the inventive linguistic capacity of primitives against their deprecators. We are familiar with his sound conclusion that languages in general exhibit about as much versatility as the demands of the culture impose. We know, as he has recently reiterated, "Elaborate distinctions are found that reflect the importance of objects and activities in the lives of the people."[1] Because the condition of snow is important to him, an Eskimo may have a dozen or more distinct words of diverse and ancient root origin to denote snow in delicately differentiated states. But in jurisprudence we do not have accepted words to so much as crudely differentiate the several law aspects of culture. This may be due to the fact that our linguistic technique, in contrast to that of primitives, is so feeble that, as Professor Boas suggests, we have had to take recourse in mere combinations of letters of the alphabet, like FDR, CIO, BMT, to convey complex groups of ideas or attitudes.[2] Or else the distinctions are not important to us, and my discourse tonight is a frivolous waste of time.

I am not capable of passing judgment on the fertility of our linguistic imagination. Nor is the lack of an adequate linguistic formulation of the different aspects of law a suitable test for the matter of linguistic adaptability to cultural needs, for the reason that social scientists, to say nothing of the non-social-science population, are not yet widely awake to the nature and importance of the meaning of law in society. Not unsymptomatic are the Lynds' famous Middletown studies of 1929 and 1937, introduced by Clark Wissler as the first application of the methods of social anthropology to a contemporary American community: a study of "the whole round of its activities."[3] As such it has won wide recognition. And yet, in this social anthropology of an American community Karl Llewellyn has of late wryly observed, "the legal aspects of behavior there did not seem worth canvass—or capable thereof."[4]

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[1] Franz Boas, "Language and Culture," in *Studies in the History of Culture*, Percy Waldon Long, editor, (New York American Council of Learned Socieites, 1942), page 178.

[2] *Ibid*, page 178.

[3] Clark Wissler, *Introduction* to Robert S., and Helen M. Lynd's, *Middletown*, (New York. Harcourt Brace and Co., 1929), page v

Among anthropologists Bronislaw Malinowski was one who bent his efforts toward a correction of this lack. In the last paper prepared for publication before his untimely death, he formulated an important detailed declaration on this very point. His article, "A New Instrument for the Study of Law—Especially Primitive," first published in The Lawyer's Guild Review[5] for May of this year, occasioned such attention and comment in the legal field that The Yale Law Journal sought and obtained permission to republish it as its lead article in June.[6] Here we have a restatement of Malinowski's theory of cultural determinism, the orders of law, and the rôles of law in the societies of man.

It will provide a convenient point of reference for the development of my thesis tonight if we first consider aspects of Malinowski's paper in some detail.

The language difficulty with which we were concerned a few moments ago sets up such an impasse that even the linguistically facile Malinowski was forced to deal with the phenomena of our interest in such artless terms of Law (1), Law (2), Law (3), and Law (4). This gave him a set of handles with which to manipulate the different facets of the complex, but the result is such linguistic drollery as the following: "When we speak of a law-abiding community, we mean Law (3). When we speak that 'law' has been brought into action, we mean Law (4). Law (4) comes into being when Law (3) ceases to work. As long as Law (3) reigns there is no room for Law (4)."[7]

What are the several meanings of these quasi-algebraic expressions?

Law (1) is used as in "law of science." With respect to culture it embraces the rules of imperative determinism imposed by "nature." These are natural laws acted upon by men but not verbalized by them, if they are primitive men. These are the laws which give form to the biologic base toward which Malinowski has insisted all culture must be functionally oriented.

Law (2) is "the rule of conduct standardized in behavior or verbally formulated." It corresponds in general to what is otherwise known as custom, and also, but not explicitly, to the sociologist's folkways. In his conception of Law (2) Malinowski clung to the idea of "neutral custom," as developed in his Introduction to Hogbin's *Law and Order in Polynesia.*[8] These are supposedly unsanctioned rules of conduct in that Malinowski defined them as involving no clash of interests of individuals or groups. The conduct they call for is taken for granted, and is so socially unimportant that no one is moved to raise a fuss, or hardly an eyebrow, when a deviant deviates from the lines of conduct set out in this category.

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[4] Karl N. Llewellyn, "The Theory of Legal Science," (The North Carolina Law Review, vol. 20, 1941), note 7a, page 7.

[5] Lawyer's Guild Review, vol. 2, 1942, pages 1-12.

[6] The Yale Law Journal, vol. 51, 1942, pages 1237-1254.

[7] *Op. cit*, Yale Law Journal, page 1244, Lawyer's Guild Review page 6

Law (3) is said to apply to rules of conduct "which refer to relations between individuals and groups, delimit divergent interests, and curtail disruptive physiological and sociological tendencies. It is the law of order and law maintained."[9] It is the law that is not broken, the law of the law-abiding community. Like Law (2) it exists without any sanctions recognizable by Professor Malinowski. It differs from Law (2) in that its rules are regulative in spheres of conduct where friction results from cross purposes and cross interests. In other words, Law (3) is positively restrictive, and successfully so.

Law (4) is defined as the specific mechanism which is brought into play when a conflict of claims occurs or a rule of social conduct is broken. It is said to be the law of retributive and restitutive social action. It is Law with a capital L.

What these four categories serve to emphasize is: that there is regularity in the social order imposed by nature and maintained by culture; and that this regularity is best seen and treated when broken down into sub-orders of law. And further, any study of these sub-orders of law is sound only when each sub-order is studied as a part of the whole and in its total relation to each and all of the other sub-orders. In other words, culture is an integrated totality, and any part of it must be studied in its complete cultural context. This is the sound functionalism which Malinowski by his works has driven home as a modern anthropological axiom. It is also a precept of the sociological jurisprudes, who are, alas, still a minority in their field: wherefore, it needs again and again to be dinned into the consciousness of men of law. This much of Malinowski's approach is good. But in its detailed orientation I cannot but feel that it is off the track, and leads not to clarity but confusion.

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[8] H. Ian Hogbin, *Law and Order in Polynesia: A Study of Primitive Legal Institutions*, (New York: Harcourt Brace and Co., 1934), page xxvi.

[9] Malinowski, "A New Instrument, etc.," Yale Law Journal, page 1250; Lawyer's Guild Review, page 5.

His great difficulty was that he abhorred the concept of sanctions—"the social restraint of violation."[10] He wanted to have his primitive man get along without them, if possible. He preferred to have his custom "neutral." The area of neutral custom in a culture is actually the area of leeway in conventions, that narrow sphere in which the individual has leave to experiment or deviate, to play with innovation. But it is a narrow area, for, after all, conventions are the "right" ways in any culture, and it may be questioned whether any society is indifferent to wrong ways.[11] Between the extremes that exist in the cultural *alternatives*, as defined by Linton,[12] lies what there is of neutral custom. Certainly, the *universals* and *specialties* of culture set standards of expectancy that are more than mere statistical norms of what people do. They are normative standards of what one ought, and even must, do. And being normative, they are sanctioned, if only by the curled lip and the raised eyebrow.

Disliking sanctions, Malinowski limited sanction to Law (4), which he treated as negative law, procedural repression. This, he said, is the legal order. In so doing, he only half perceived the legal order; he saw the action but not the substance. I have in mind what the law professors identify when they speak of adjective law and substantive law. Substantive law consists of the social rules of conduct to be followed by law-abiding men in the give and take of daily life. They are the rules to which adjective law applies so as to channel behavior that men shall go around in certain ways in certain spheres of activity. They define demand-rights and duties, powers and lack of powers, privileges and lack of demand-rights, immunities and liabilities.[13] These substantive rules all fall in the category of Malinowski's Law (3). The legal order, Law with a calital L, therefore quite definitely laps over into Law (3), even though Malinowski claimed it is restricted to Law (4). But not all of his Law (3) is a part of the legal order, as I shall attempt to show later. For the moment, we may say that the body of substantive law consists of social norms, or rules for conduct, which define what a person may and may not do with legal impunity, or conversely, what he must, or must not do, if he would avoid legal penalties. It enunciates the requirements of performance and restraint from performance. It consists, therefore, of the norms which are sanctioned by adjective law. It sets the flags in the slalom run of life, if you want a slippery metaphor.

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[10] Robert I. MacIver, *Society Its Structure and Changes*, (New York, Ray Long and Richard Smith, 1931), page 249.

[11] William Graham Sumner, *Folkways, A Study of the Sociological Importance of Usages, Manners, Customs, Mores, and Morals*, (Boston: Ginn and Co., 1913), pages 28 ff.

[12] Ralph M. Linton, *The Study of Man*, (New York. Appleton Century, 1936), pages 271 ff.

[13] For the exact meaning of these terms and their use, see, E. Adamson Hoebel, "Fundamental Legal Concepts As Applied in The Study of Primitive Law," (The Yale Law Journal, vol. 51, 1942), page 954 ff.

If the substantive law were never broken, there would then be no call for an enforcement machinery. But it is broken betimes, and adjective law embodies the rules of how the dastardly culprit is to be dealt with. It is the procedural law of right modes of action directed against wrong-doers, real and alleged.

The point for which I am reaching is that Professor Malinowski, it seems to me, confused rather than clarified the nature of the legal by limiting it to adjective law solely. It is essential to understand that adjective law is only half of the Law. Adjective law without substantive law is unthinkable. When it does exist, it is only in the abortive tyranny of an Hitlerian Gestapo, which knows no law but its own tortured whimsy.

If Malinowski's Law (3) and Law (4) do not satisfactorily accord with the facts of what constitutes the legal, what alternative concepts may be suggested as more useful?

First, I hope it is clear from my remarks up to this point, that the legal order is to be seen merely as one aspect of the mechanisms of social control. Not all social norms are legal norms. Some of them are nonlegal (custom) and others are by-legal. But first, it is probably high time I make clear *what* should pass as legal.

A feature of all social norms, sub-legal and legal, is of course, regularity and expectancy. Norms are what people regularly do and what you count on them doing in the future. Legal norms have regularity, imperative authority, and, in addition, an element of officialdom. A norm, if it is legal, is a must. It is an imperative buttressed by coercive force to be called upon at need. Law has biting teeth, not always bared, but still having the power to rend and destroy, if necessary.

It is essential that we insist upon recognition of the absolute imperative, if we are to make the anthropological treatment of law meaningful and realistic. We must check the anthropological tendency to spread the concept of law over the entire body of social control. To deny that law is limited to the operation of courts of the state is anthropologically sound, but to say that universal custom is law is evidence of muddy thinking. For example, Chapple and Coon's notion that a law is a symbolized "pattern of action for all the members of a group," while custom "is limited to the members of an institution or to part of an institution,"[14] will get us nowhere in effecting an understanding of what is legal. Similarly vague and overbroad, Driberg has approached the analysis of law in East Africa with the idea that, "Law comprises all those rules of conduct which regulate the behavior of individuals and communities."[15] Gillin in his treatment of Carib law suggested an ephemeral extreme with the declaration that law, broadly considered, is simply that body of opinion within a group which regulates the behavior of the members of the group.[16] It may be happily noted that this custard pie concept of law does not make its way into the recent sociology by the team of Gillin and Gillin.[17]

Against these over-extended anthropological vagaries, consider the legal scientists' expression on the specificity of law. By the English analytical authority, Salmond, rules of law are said to be, "The principles enforced by the State through judicial authorities by physical force in the pursuit of justice whether attained or not."[18] From the great and liberal German, Jhering, we learn that, "Law without force is an empty name."[19] "A legal rule without coercion is a fire which does not burn, a light that does not shine."[20] From the "pure theorist," Kelsen, comes the insistence that law in essence is "an external compulsive order."[21] And from our own great genius of the law, Holmes, "The foundation of jurisdiction is physical power."[22]

We shall err if we do not follow the students of jurisprudence in their analysis of coercive sanction as an important element in law.

In addition, we must reach a clear understanding of the element of officialdom. To the orthodox political theorist, legal philosopher, or lawman the official element in law means a state operating through courts. Here the anthropologist has a justifiable say in demanding that the interpretation of state and courts be wide indeed. It was in response to the demands of anthropological facts that Professor Max Radin was led to observe in reference to the official element in law, "But there is an infallible test for recognizing whether an imagined course of conduct is lawful or unlawful. This infallible test, in our system, is to submit the question to the judgment of a court. In other systems, exactly the same test will be used, but it is often more difficult to recognize the court. None the less, although difficult, it can be done in almost any system at any time."[23] The ambiguous court, difficult to find in some systems, i.e., primitive systems which operate without institutionalized courts manned by a selected personnel of specialists, may be the peoples' court of overtly expressed acceptance or rejection of the claim of a litigant as laid before the bar of public opinion. Remember, in a small primitive community all litigation of claims gathers a crowd of spectators, whose approval or disapproval of the substantive grounds for the claims advanced by each litigant is expressed in one way or another.

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[14] Elliot D. Chapple and Carleton S. Coon, *Principles of Anthropology*, (New York: Henry Holt & Co., 1942), page 658.

[15] J. H. Driberg, "Primitive Law in East Africa," (Africa, vol. 1, 1928) page 65.

[16] John Phillip Gillin, "Crime and Punishment among the Barama River Carib of British New Guiana," (American Anthropologist, vol. 36, 1934), page 140.

[17] John Lewis Gillin and John Phillip Gillin, *Sociology*, (New York: The Macmillan Co., 1942). See the discussion on page 539.

[18] John William Salmond, *Jurisprudence*, (London: Sweet & Maxwell, 1924).

[19] Rudolph von Jhering, *Law as a Means to an End*, (New York. The Macmillan Co., translated by Isaac Husick, 1924), page 190.

[20] *Ibid.*, page 241.

[21] Hans Kelsen, "The Pure Theory of Law," (The Law Quarterly Review, vol. 50, 1934), page 48.

[22] Oliver Wendell Holmes, *McDonald v. Mabee*, (United States Reports, vol. 243), page 90; (United States Supreme Court Reporter, vol. 37), page 343.

In a primitive system of law, the official element means that any thing done or commanded in terms of the Law is done, or commanded, for the well-being of the community. This is wholly true even in systems of so-called private law, or self-help. The aggrieved husband, out to punish the adulterous transgressor of his private marital privilege-rights, if he proceeds in accordance with established and recognized rules, has at least the tacit backing of general social approval. This gives him for the occasion an implicit social authority, and his acts are thereby quasi-official. In a modern law system, in contrast to most primitive ones, only a duly authorized and properly installed state official has the power to exercize the coercive legal imperatives. In our civil law (the field of private wrongs, or torts) the aggrieved party must take the initiative in instituting legal action, but judgment on his claim and the execution of the judgment are reserved for the legal authorities. The provision of judicial and police officials to protect the demand-rights of the private citizen is our token of the public interest in, and recognition of, the aspect of general social well-being in the private claim of the aggrieved.

It is in this sense that the point is made that there is no absolute distinction between private and criminal law in either primitive law or modern.[24] This is not intended to maintain, as Professor Lowie has interpreted the argument, that there is no value in distinguishing crime from tort. We must agree with him that "the crystalized sense of public wrong" and "procedure by an agent of the state" are not minor matters.[25] In fact, the whole sweep of law in its evolution from primitive to modern has been in the direction of the supersedure of private law by public. But primitive law, which consists predominantly of private law, can be understood only by recognizing and insisting on the importance of the fact that the private prosecutor of a tortious transgressor is an implicit public official *pro-tempore, pro eo solo delicto.*

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[23] Max Radin, "A Restatement of Hohfeld," (Harvard Law Review, vol 51, 1938), page 1145.

[24] Cf., Karl N. Llewellyn and E. Adamson Hoebel, *The Cheyenne Way: Conflict and Case Law in Primitive Jurisprudence*, (The University of Oklahoma Press, 1941), pages 47–50.

[25] Robert H. Lowie, Review of *The Cheyenne Way*, (American Anthropologist, vol. 44, 1942), pages 478-479.

In recapitulation, that which is characteristic of law is: (a) regularity—a common feature of laws of science and all social norms, legal and non-legal; (b) imperative sanctions, law is a "must" with biting teeth; and (c) an element of officialdom—recognized acceptance of the procedure and the prosecutor as operating for good of the society as a whole.

Now what about law-ways? This term is designed to give expression to the whole complex of behavior patterns which go to make up a legal system. It is therefore intended to include not only the laws themselves but also the ways of doing in conjunction with these laws; the ritual of opening a court session, the word formulas of legal lingo, the magical over- and undertones of legal institutions, principles of logic, and the techniques of pleading—all patterns of behavior which provide the connective tissues that give shape to the legal structure. Law-ways are the laws and what people and officials *do* about the law.

Another concept offered for social science use is law-stuff. Not an elegant sounding word, but it has its value. Law-stuff emerges from the matrix of human activity not on the level of law and law-ways but which, by its nature, generates law-ways and law. It is troublesome behavior in the frictional area of clashing interests and dangerous deviations from the norms of the group. It is what Malinowski had in mind when he spoke of Law (3). The lawyer and the legal scholar have not ordinarily concerned themselves with the study of law-stuff, but the anthropologist and sociologist must analyze it carefully. For it is in this area that the sociology of law makes its great and unique contributions. *We* can effectively study law-stuff because our ideal is to see behavior in all its aspects. And since the genesis of law lies in law-stuff, we can contribute greatly to the growth of a more realistic understanding of the nature and social function of law by paying close attention to law-stuff.

There is one other aspect, and a complex one, of Primitive Law and Modern to which I would like to allude before I give expression to my closing thoughts. This is the by-law of sub-groups within the social order. Every established primary and secondary group has its code,

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[20] Llewellyn and Hoebel, *op. cit.*, pages 26 *passim.*

consisting of by-laws, applicable to its own members but not to non-members.

The university has its code of fines for late registration and missed examinations, its penalties for dishonorable conduct. The family has its rules for proper conduct at the table, and father (at least some fathers) has authority for enforcement. The clan has its demand-rights against its members for performance of clan duties, and the clan headman or the council has authority for enforcement. The Plains military societies had the power to punish members who failed in their duties to the society. Secret societies of Africa and Melanesia may destroy members who reveal their secrets to the non-initiate. The Church can banish and excommunicate. The demands of by-law can be as pressing as the demands of law. Nevertheless, with due allowance for sporadic exceptions, law prevails over by-law, if it is law in fact. Law speaks for the entire society, while by-law expresses but the interests of a part of the social whole.

This leads us to the lesson which the study of primitive law has to offer for the future. Today we have International By-law but no International Law. On the international level primitive law prevails. And only the most incipient type of primitive law at that! What has passed as international law consists of no more than normative rules for the conduct of affairs between nations as they have been enunciated and agreed upon from time to time by means of treaties, pacts, and covenants. In addition, prevailing custom in international intercourse, recognized by tacit consensus or verbalized in arbitration and world court awards, provides the other main source of its substance. But this body of social norms for international intercourse is as yet no more than the by-laws of the sub-groups we call nations.

International law, so-called, has consisted of substantive rules without imperative legal sanctions. This nation and our people now at war fervently and sincerely believe that we are serving humanity as an enforcer of international law and as a righteous protector of justice and human decency. It is morally right that we should feel so. But our claim to this status would be clearer if the element of world officialdom had been bestowed upon our cause more explicitly by an established world judiciary.

The late League of Nations was created to provide for this need. It gave to international rules the machinery and the personnel of a recognized officialdom. And yet, because the power of universal coercive, absolute force was withheld from it, it could not make law of the international norms. Under the League international law was a fire which did not burn, a light that failed to shine.

Whatever the idealist may desire, force and the threat of force are the ultimate power in the determination of international behavior, as in law within the nation or tribe. But until force and the threat of force are brought under the socialized control of the world community, by and for the world society, they remain the instruments of social anarchy and not the sanction of world law. That the world has entered the stage of community consciousness in which the people cry out for an organized world law, that there may be world order, is evidenced by the fact that even the Great Disturbers lay the claim before the bar of public opinion that their New Order is to be an order and world regularizer.

We set against their preposterous claim the Four Freedoms as the ideal basis for a true international law. The Four Freedoms offer a goal of deep and immediate emotional appeal, a worthy aim for true men. Yet, as we all know but sometimes forget, the attainment of goals, however worthy, requires the creation and utilization of means. The inevitable instrument we must create, that the Four Freedoms may be realized and maintained, is a true international adjective law. The inescapable corollary of this realization is the creation of a functioning instrument of world decision to speak the law, and the establishment of a supernational armed force that may not be successfully challenged by any nation or minority of nations. It must be a force responsible to the World Commonwealth alone.

To do this we must adjust our psychology to that of world community. Anthropologists do this naturally. We must bend our efforts harder and harder to teach others likewise. Let us drive home the lesson that, "without law there is no order and without community there is no law."

The metamorphosis from primitive law to modern on the plane of international intercourse awaits the emergence of the consciousness of world community by all men. The immediate and unmistakable conclusion from the study of the anthropology of law is that this metamorphosis must and will be turned. The only question is—when? If fulfillment comes in our day, it shall be our happy destiny to participate in the greatest event in legal history.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 5 JANUARY, 1943 No. 3

SECTION OF BIOLOGY[1]

DECEMBER 14, 1942

PROFESSOR THOMAS HUME BISSONNETTE, Head of the Department of Biology, Trinity College, Hartford, Connecticut: *Some Recent Studies on Photoperiodicity in Animals.* (This lecture was illustrated by lantern slides.)

HISTORICAL

Bissonnette[1] showed that the pelt-cycle of ferrets or fitch were photoperiodic reactions related to cycles of the anterior pituitary and controllable by appropriate daily lighting. Bissonnette and Wilson[4] found that pelting-time in the mink could also be controlled by similar light-control and winter prime pelts secured in August and September, instead of November, by appropriate reduced daily illumination. But neither of these animals is good for a careful analysis of the times and courses of the molts and growth of new hairs by which changes of color of pelt or pelage are brought about in mammals, or of plumage in birds, because the new coat is very similar to the old one in color in both spring and autumn.

Weasels were then chosen for the experimental study of the factor or factors in the environment the changes of which induce the spring and autumn molts and replacement of fur. The short-tailed or Bonaparte weasel (*Mustela cicognanii cicognanii* Bonaparte), in Connecticut,

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[1] The Section of Geology and Mineralogy was addressed on December 7, 1942, by Doctor Paul D. Krynine on *The Evolution of the Sedimentary Rocks during a Diastrophic Cycle* (Illustrated by lantern slides.) No abstract was submitted for publication. No meetings were held in December by the Sections of Psychology and Anthropology.

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Executive Secretary: Eunice Thomas Miner.

Assistant Editor: Ethel J. Timonier.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

changes to white or near white in winter, while the long-tailed or New York weasel (*M. frenata noveboracensis* (Emmons)) changes to a lighter shade of brown (Goodwin[7]; Hamilton[8]). Three Bonaparte and eight New York weasels were secured from a trapper in Pennsylvania from a single neighborhood. One was an abnormal male Bonaparte weasel that apparently controlled its own light-cycles while on normal daylight cycles with controls for two years and also when on experimentally controlled lighting. The vagaries of its molts and changes of color have been described by Bissonnette.[2] The other Bonaparte weasels and the New York weasels underwent the usual pelt-changes at the usual times when on normal light-cycles and were therefore considered to be normal in these respects and strictly comparable for experimental purposes.

The conflicting conclusions of recent workers on this general problem may be pointed out. Miyazaki[12] found that shortening days induce molting in the Japanese Mejiro bird, or white-eye, and that increased lighting in the molting period led to irregular and abnormal regrowth of feathers. Witschi[16] concluded that altered environmental conditions, in Iowa, including lighting, do not affect plumage-cycles in the weaver finch (*Pyromelana franciscana*) which depend upon cyclic hypophyseal activity. Brown and Rollo,[5] in Illinois, experimentally modified the plumage-cycles of whydahs and weaver finches (*Steganura paradisea, Pyromelana franciscana*, and *Vidua principalis*). The first two species do not normally come into nuptial plumage until two years of age. But, on sixteen hours of light per day, all three species came into nuptial plumage in the first year, with *Pyromelana* most, and *Steganura* least resistent to light treatment. On sixteen-hour light-periods, a *Pyromelana* remained in nuptial plumage for over a year, while a control, on normal daylight, went into eclipse just over a month after the experiment began in December. A yearling male on restricted daylight periods, in process of molting from eclipse to eclipse, very soon began to regenerate nuptial feathers when subjected to sixteen-hour light-periods. Type of feather regenerated in a male *Pyromelana* in nuptial plumage, after plucking, was affected by duration and intensity of daily light-periods.

Walton[15] caused mallard drakes to take on eclipse plumage in February–March instead of the normal time in June–July by artificial lighting in addition to normal light-periods. Complete castration did not prevent this in the first year but did in the second. He concludes that eclipse is not caused by testicular hormone directly acting on the feather follicle, but that removal of the testes sooner or later affects some other endocrine organ or organs which consequently become insensitive to seasonal change (light) or fail to induce eclipse.

Witschi and Miller[17] found that sex-hormone injections do not affect plumage in European starlings (*Sturnus vulgaris*), indicating that their plumage-cycles are independent of the gonads. Burger[6] found that molting and subsequent refeathering in starlings will occur irrespective of day-length; but a reduction of day-length favors or speeds up these processes. Their inherent rhythm is modifiable by altered lighting.

Lesher and Kendeigh[10] found that *Zonotrichia albicollis* and *Colinus virginianus*, birds with a normal prenuptial molt in spring, will molt out of season if the daily photoperiod is raised to fifteen hours; but *Passer domesticus* and *Junco hiemalis*, normally without prenuptial molt, fail to do so. *Zonotrichia*, *Passer* and *Colinus*, that normally have complete postnuptial molt in late summer, molt similarly in early spring after photoperiod is reduced from fifteen to nine hours, either immediately or by fifteen or by five minutes a day. The time that molt begins and rate of loss of feathers is accelerated at the faster rates of light-reduction. Time taken in renewal was not affected. Appropriate change of photoperiod appears to induce molts of spring or autumn type only in birds normally having that type of molt, not otherwise. To molt or not to molt, on a given change of day-length, is a specific inherited character.

Salomonsen[14] described the plumage and color-changes of the rock ptarmigan (*Lagopus mutis* (Montin)) from birds in nature and pelt-changes in stoats, weasels, and varying hares, among others, from museum specimens and skins in correlation with average temperatures at regions and dates of capture. From these data, without experimental studies, he concluded that change to low temperatures conditions and controls the change to white in these birds and mammals, with varying susceptibility to this change of temperature determining time and degree of whitening with the species. Reduced illumination was dismissed as of minor or no importance. The course of the change in the mammals was not followed, or described in detail. Per Hóst,[9] by experimentally controlled lighting, induced changes of plumage color and molting in the willow ptarmigan (*Lagopus lagopus*) irrespective of high or low temperatures. He caused birds to whiten out of season and to return to normal spring plumage and to lay eggs in winter, by appropriately controlled lighting, in Norway. The birds went through their series of molts or altered them, even skipping some, depending on the type of lighting regime.

Lyman,[11] using the varying hare (*Lepus americanus*), found that reduced daily lighting induces molting of brown coat and growth of white hair in autumn; increased daily light-period induces molting of white coat and growth of brown hairs in spring. Either of these changes of color can be induced out of season by proper lighting. He found a pre-winter physiological condition giving a minor molt from brown to brown before the major molt from brown to white. He plucked areas on the shoulders and noted the color of regrowth, and so did not follow the wave of change over the animals nor the effects of quickly reversed changes of lighting. He found that either castrated or thyroidectomized hares responded like entire animals to the light-manipulation. So it appears that neither the sex-glands nor the thyroids are essential to the molts or color-changes in hares.

Miss Rothschild,[13] using the stoat or English ermine (*Mustela ermmea*), found that she could not induce or initiate molts by changes of temperature; she could hasten the completion of the winter molt and growth of white hair in some animals by lowered temperature, causing change to white to be completed in seventy hours. Not all stoats, even from the same litter, became white in winter under similar conditions of temperature, which seemed to control the whiteness of the coat. She described the course and order of whitening of parts of the animal and pointed out that both seasonal changes are by complete molts and growth of new hairs.

Our experiments, described below, on the Bonaparte and New York weasels, give results in agreement with those of Lyman, Hóst, Lesher and Kendeigh, Burger, Bissonnette, and Bissonnette and Wilson, on nearly related species. It may be said here that the New York weasel agrees with the Bonaparte weasel in cause and times of molts, but fails to become white both normally and experimentally in this locality under the conditions of our experiments. Effect of cold upon speed of autumn molt was not tried.

Local races of weasels have apparently been selected out which behave differently as regards change to white in winter although of the same species. In Connecticut, the New York weasel changes merely to lighter brown in winter; but farther north in the boreal zone and northern New York State, about fifty per cent turn white with a sex difference in that it is the males that fail to turn white and females do so.[8] We find that the males are more venturesome than females and spend more time outside their dens or looking out. This leads us to suspect that less reduction of light in autumn, in spite of greater cold in the open, enables the males to be less affected than the females that more often stay in the dens, rather than less reduction in temperature or loss of sight of snow. The latter is believed by some to be the cause of whitening in those animals that become white in winter. If cold or sight of snow were the determining factor the males should be the ones to whiten. Cold may cause the females to stay in the dens and so reduce their daily light-period more than that of the males. In that way it might indirectly help to induce whitening by reducing daily light-time.

Dr. Raymond Hall, of the University of California at Berkeley, pointed out in a letter that *Mustela frenata* at Berkeley remain brown in winter, while the same species at Salt Lake City, Utah, become white, as do those at Lake Tahoe, in the Sierra Nevada. Transfers from Berkeley to Lake Tahoe remain brown in winter in the new locality, and transfers from Salt Lake City to Berkeley turn white as they did at their original home site. This all points to the necessity of using animals from one locality and similar in normal behavior for experimental studies of this sort, lest differences in normal behavior carry over into the experimental results.

MATERIAL AND METHODS

On the working hypothesis that change of daily illumination is the factor concerned in these color reactions, the time and rate of change of daily lighting was altered experimentally for one Bonaparte and four New York weasels while the other two Bonaparte and four New York weasels served as controls on normal light-cycles, to see if time and rate of change of pelt in both species would be changed thereby. All were kept in the same room under like conditions of temperature and dark-colored surroundings at Hartford, Connecticut. The animals all came from the same locality in Pennsylvania. On two occasions controls were moved to a warmer room while experimentals were subjected to additional lighting after dark, but in these cases the warmed room would act against the added lighting if temperature were a factor in the results. Temperatures in the experimental room were not rigidly controlled but showed little or no seasonal variation except greater daily fluctuations when the heating system was operating than in summer when it was not. In general it was near a mean between maximum and minimum daily temperature in summer and above both in winter. The arrangements were such as to rule out temperature changes and the sight of snow or similar bright surroundings as factors in the results or deviations of experimental animals from the controls, because these are the two factors most often given as the causes of change to white in winter for such animals and return to dark colors in spring. Food and care were alike for both groups of animals. The den and runway system in use was that described by Bissonnette and Bailey[3] and a small darkroom built in the large room was used to shorten days below normal and electric lights to prolong them beyond normal when called for in the experiments.

Photographs of experimentals and controls were made for comparisons at suitable times and to follow the wave of change over the body and extremities of the animals.

For the experimental group, the daily period of light was reduced by one-half hour each *five* days from May 23, 1939, to seven hours on July 20, then to six hours on September 20; kept at that period until November 12; then increased by one-half hour each *four* days to twenty hours, February 27 to March 2, 1940; then reduced again at the same rate to one hour per day on July 30–August 2; then increased likewise to twenty hours, December 28, 1940–January 6, 1941; and reduced directly to normal day-length of about nine hours and thirteen minutes. From that day onward these animals were on normal days, as the controls had been up to this time.

On April 12, 1941, the surviving two Bonaparte controls and one female New York control were treated to experimental light-manipulation, while the surviving Bonaparte experimental and a male New York control were left on normal days as controls, until December 1, 1942. In this way each animal served as control against itself in two or three seasons and as experimental against normal controls as well throughout. These new experimentals were subjected to reduction of daily lighting by one-half hour each four days to one hour per day, July 13–16, inclusive, then to additions at the same rate to nineteen hours December 4–7, then by one hour each four days to twenty-four hours, December 24–January 1, 1942; then reduced one hour each four days to fifteen hours, February 3–6; then directly to normal days, February 7–March 10; then to four hours per day, March 11-24; then to one hour March 25–April 10; and back to normal days of approximately thirteen hours from April 11 onward. These changes were to test whether it is change of day-length that is the factor or mere length of day itself that induces the changes in color.

Results and Conclusions

Temperature change was not an appreciable factor in the observed modifications of times of molt, pelt and color cycles, or degree of molting in these animals. It is therefore concluded that temperature-cycles do not control molting or color-changes in these weasels except perhaps indirectly as suggested below. Neither is color, nor change of color, nor whitening of surroundings in control.

Reducing daily periods of light, either gradually or suddenly, induced molting and regrowth of new fur in varying degrees for both species of weasel: in the Bonaparte, white replaced brown, and in the New York weasels, lighter brown replaced dark brown on the body and some white appeared on the feet. The black tail-tips remained black throughout. (In what follows read light brown instead of white for New York weasels.)

Increasing daily light-periods caused molting and change to dark brown in varying degrees over the bodies in both species.

Incomplete molts and change of color in both directions (toward white or toward brown) were produced as a result of early reversal of increase or decrease of daily light-time. This sometimes left the animal for some months at a time with parts of its body showing hairs of each of three consecutive growths of hair.

Even on normal days, molts and change of pelt in either direction are not always complete, and vary in the same animal. Thus "gray-backs" in winter and animals with some white in places on the body usually turning brown in summer occur among controls. "Gray-backs" also occur in nature and probably so do cases of incomplete summer browning. These are quite easily accounted for if daily light-periods control molting and change of color.

Usually the autumn molt and change to white hairs comes as a wave over the body from below upward and along the tail from below and base to top and tip, with the last parts to turn white being a spot between the ears and down the nape of the neck, with occasional spots on the hips. This is much better adhered to than the usual method of change to brown. The latter usually begins around the lips and along the ridge of the back and top of the head and sweeps downward on the body and backward on the tail toward the tip. But if change to longer days comes before the white fur along the back is completely grown in, the brown may appear along the sides and flanks and spread upward and downward and leave the back of the animal and a ring on the tail next the black tip, and some of the legs and feet and face, white through the period of the otherwise brown coat. The same animal may react in either this manner or the normal one in different seasons. Animals may be induced to molt from white into white or from brown into brown by proper lighting. The colors need not alternate with each other. Animals were made to assume white coats in spring at about the same time as controls were becoming brown; and to become brown before Christmas after whitening early in the autumn. Two cycles of color-change were obtained in about one year instead of the one that is normal, by subjecting the animals to two light-cycles in the year.

A latent or adjustment-period always intervenes between the time of increase or decrease of light-ration and the shedding and regrowth of hair. This appears to be about three months or less. Whether this is amenable to experimental change of duration is not yet clear. The internal reaction-system takes time to adjust to stimulating change, if this is a sudden large reduction of increase in daily light-time or not.

Not all animals of the same species react alike, even if secured from the same locality, and the same animals react differently in different seasons or years. Local races in which a majority react alike are probably produced by natural selection, such that in regions with snowy winters a majority turn white in autumn and continue to do so after removal to regions where there is little or no snow in winter, and a majority remain brown in winter in regions where snow is scarce and continue to grow brown winter coats when removed to regions with winter snow. Studies on this general problem should therefore be made with animals known to react alike in a given season and from the same local race. In such cases, the place or locality where experiments are carried out is of little or no importance

After light-manipulation and consequent modified color-cycles lasting through two years or longer and several color-changes, the animals return to normal pelt-cycles after return to normal light-cycles in a normal direction or manner; but not if such return is abnormal, i.e., too abrupt or in an unusual direction. Return to normal days from longer days induced whitening in the Bonaparte weasels; from shorter days, browning, in spring or summer.

That this stimulus is received through the eyes and acts through the anterior pituitary is indicated from Bissonnette's studies with ferrets[1] and that the thyroids and sex-glands are not essential to it is at least suggested, if not indicated, by Lyman's[11] study on the varying hare.

These results and conclusions are in agreement with those of Höst,[9] Brown and Rollo,[5] Lesher and Kendeigh,[10] Miyazaki,[12] Walton[15] and Burger[6] from studies on birds, and suggest that this phenomenon is of quite broad distribution among birds and mammals. That this is invariably the controlling factor in such changes is by no means demonstrated. As with sexual periodicity, it is probable that other factors may operate in other animals to bring about the synchronization with the seasons. Temperature reductions, or any other factor or factors which drive the animals into their dens or underground into the dark for longer hours, may indirectly induce the taking on of white or near white coat in autumn, and the reverse may induce change to brown in spring. Changed habits of the animals in this regard may be self-imposed and bring about unusual times and degrees of change of pelt-color or molts.

These studies were aided by grants from the Penrose Fund of the American Philosophical Society, 1938–1939, and from the American Academy of Arts and Sciences, 1942. The paper on the work *in extenso* will be published elsewhere under the authorship of Thomas Hume Bissonnette and Earl Elmore Bailey, Trinity College, Hartford, Connecticut.

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REPORT OF THE ANNUAL MEETING

DECEMBER 15, 1942

The 125th Annual Meeting of the Academy for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at The American Museum of Natural History on the evening of Tuesday, December 15.

The Corresponding Secretary reported that there are now 44 Honorary Members and 4 Corresponding Members upon the rolls of the Academy. Four deaths were reported during the past year.

The opening paragraph of the Recording Secretary's report of last year serves as an excellent beginning for this year: "The most outstanding fact regarding the Academy this year is the continuance of its substantial progress, in spite of existing world conditions."

Celebrating the 125th year of its founding, the Academy has maintained and, in some respects, improved upon the records previously established. The continuance of its program of progressive activities, especially that part concerned with the meetings and publications, is a noteworthy accomplishment in, this, our first year of war.

Plans for the extension of our progressive program have been made with a keen realization that science, like all other educational and cultural phases of civilization, must play its part by making the greatest possible contribution in ideals and achievements toward future world stabilization.

The Council is deeply appreciative of the support and cooperation given the Academy by its Members.

During 1942, the Academy held 27 Regular Sectional Meetings, at which 27 stated papers, all of high caliber, were presented, in addition to the Annual Business Meeting and Dinner, at which two excellent addresses were delivered. Six informal receptions were held under the auspices of the various Sections of the Academy.

The Section of Oceanography and Meteorology held a conference on "Boundary Layer Problems in the Atmosphere and Ocean." This symposium was attended by a large number of outstanding workers in these fields and initiated the activities of the new Section.

The Section of Physics and Chemistry held three conferences on special subjects of research, under the following titles: "Protein Hormones of the Pituitary Body," "Physics of the Solid State," and "Color, Constitution and Reactions of Dyes."

The membership of the Academy not only has been sustained successfully in financial support, but, in this respect, has improved its record by an increase of income. Although a small net loss of membership is shown, it seems advisable to point out that this is due to the termination of membership for those not in good financial standing during the past two years.

Two Honorary Members, 2 Life Members, 116 Active Members, 46 Associate Members and 2 Student Members were added to the rolls, of which 156 are now in good standing and 12 await qualification through payment of dues. Thus, a total of 168 new Members were added during the year.

The present membership of the Academy is 1,964 Members, of which there are 259 Fellows, 1 Patron, 105 Life Members, 134 Sustaining Members, 1,017 Annual Active Members, 634 Associate Members, 25 Student Members, 44 Honorary Members and 4 Corresponding Members.

The Editor reported that during the fiscal year of 1942, the Academy has published 778 pages as of this date. Of this amount, 382 pages were published in the Annals, 258 pages in the Transactions, and 138 pages in the Scientific Survey of Porto Rico and the Virgin Islands. It is also appropriate to announce that the 125th Anniversary Volume, being Volume II of Special Publications, is printed and now being bound, and will appear before the end of December, 1942. This comprises xvi, plus 178 pages and 100 plates, bringing the total pages to 1072. In addition, Article 6 of Volume 43 of the Annals is now in galley proof, and Article 1 of Volume 44 of the Annals has been sent to the printer in manuscript form. These two papers will comprise some 280 pages, to appear early in 1943.

An itemized list of these publications follows:

ANNALS

Volume 42, Article 4—"Intelligence in Mental Disorder," by Anne Roe and David Shakow. Pages 361–490. Published January 31, 1942.

This completes Volume 42.

Volume 43, Article 1—"Thermodynamic Properties of Solutions of Long-Chain Compounds," by Maurice L. Huggins. Pages 1–32. Published March 30, 1942.

Volume 43, Article 2—"Immunochemistry," by William C. Boyd, Enrique E. Ecker, Michael Heidelberger, Sanford B. Hooker, Forrest E. Kendall, Stuart Mudd, L. Pillemer, Joseph E. Smadel, Theodore Shedlovsky, and Charles A. Zittle. Pages 33–122. Published April 30, 1942.

Volume 43, Article 3—"Studies on Fresh-Water Bryozoa. XII. A Collection from Various Sources," by Mary D. Rogick and Claudeous J. D. Brown. Pages 123–144. Published May 12, 1942.

Volume 43, Article 4—"An Analysis of the Locomotion of the Seahorse, *Hippocampus*, by Means of High Speed Cinematography," by C. M. Breder, Jr., and H. E. Edgerton. Pages 145–172. Published June 20, 1942.

Volume 43, Article 5—"The Ultracentrifuge," by D. A. MacInnes, W. J. Archibald, J. W. Beams, W. B. Bridgman, Alexandre Rothen and J. W. Williams. Pages 173–252 Published October 6, 1942.

Volume 43, Article 6—"Protein Hormones of the Pituitary Body," by H. B. van Dyke, Bacon F. Chow, Vincent du Vigneaud, H. L. Fevold, George W. Irving, Jr., C. N. H. Long, Theodore Shedlovsky and Abraham White. Pages 253–000. *In galley proof*

This will complete Volume 43.

THE SCIENTIFIC SURVEY OF PORTO RICO AND THE VIRGIN ISLANDS

Volume 19, Part 1—"Meteorology of the Virgin Islands," by Robert G Stone. Pages 1–138. Published October 28, 1942.

TRANSACTIONS

Transactions, Series II, Volume 4, constituting a 258-page volume in 8 numbers, has been printed and distributed

SPECIAL PUBLICATIONS

Volume II "Balinese Character, A Photographic Analysis," by Gregory Bateson and Margaret Mead. Pages xvi, plus 178, and 100 plates of photographs. This volume is in commemoration of the 125th Anniversary of the Founding of The New York Academy of Sciences Published December 7, 1942.

The Librarian reported that 13,309 separate publications and 2,080 complete volumes of Transactions, Series II, Volume 4, were distributed to the members of the Academy, authors, exchanges and the sales list, classified as follows: *Annals*, 7,975; *Scientific Survey of Porto Rico and the Virgin Islands*, 562; *Special Publications*, Volume I, "Climate and Evolution," 29; and *Transactions*, Series II, Volume 4, complete volumes, 2,080 and 4,743 single copies.

The Library of the Academy has received 913 separate publications from exchange institutions.

The Treasurer reported that considering the difficulties that war brings to all institutions, often causing them severe hardship in the maintenance of their activities, the Academy has made a remarkably good record this year.

The total income received from all sources amounted to $22,750.31.

Receipts from sales of publications, amounting to $2,679.87, shows an increase, during 1942, as compared with the total publications sales of last year.

The Members of the Academy, showing an ever progressing interest in the Academy's welfare, added 168 new names to its rolls, thus producing a gain of $104.25 in revenues from dues above that of last year, making a total of $7,856.54 from this source. This is exclusive of $400.00 received for Life Memberships.

The investments of the Academy have remained the same during the past year, except for the sale of certain stock rights. The investment portfolio was carefully watched and it was deemed wise to make no changes at this time.

The book value of the Academy's property, as of November 30, 1943, amounts to $92,490.49, of which $76,787.67 is in stocks and bonds, $14,770.15 is in real estate and $920.07 is in a mortgage bond.

As usual, the books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1942, and audited by the Finance Committee, as provided by the Constitution and By-Laws. The property of the Academy was verified and the Treasurer's Report examined and attested by the Finance Committee as of December 14, 1942.

The A. Cressy Morrison Prizes of $200.00 each for the two most acceptable papers in any field of natural science, within the scope of the Academy and its Affiliated Societies, were awarded to the following papers,[1] entitled:

"New Methods in Stellar Dynamics," by S. Chandrasekhar, Yerkes Observatory, Williams Bay, Wisconsin, and "Comparative External Anatomy, Phylogeny, and a Classification of the Bees," by Charles D. Michener, The American Museum of Natural History, New York, N. Y.

Honorable Mention was given to the papers entitled, "The Relation Between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails," by Arthur W. Pollister and Priscilla F. Pollister, Department of Zoology, Columbia University, New York, N. Y., and "The Haplolepidae, a New Family of Late Carboniferous Bony Fishes," by Doctor T. Stanley Westoll, Department of Geology and Mineralogy, University of Aberdeen, Scotland.

The Committee takes great pleasure in congratulating the authors of these papers and desires to express its appreciation of this method of bringing to the attention of the Academy valuable and outstanding records of scientific research.

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[1] Abstracts of these papers are included in this issue of the Transactions.

The following members were elected to Fellowship:

Donald H. Andrews, Ph.D.
Gregory Bateson, M.A.
Robert M. Burns, Ph.D.
Bacon F. Chow, Ph.D.
Thomas Francis, Jr., M.D.
Julio Garcia-Diaz, Ph.D.
Otto H. Haas, Ph.D., LL.D.
Arthur J. Hill, Ph.D.
Columbus Iselin, 2nd, A.M.
Joseph L. Johnson, M.D., Ph.D.
Alois F. Kovarik, D.Sc., Ph.D.
Perrin H. Long, M.D.
Joseph E. Mayer, Ph.D.
Eunice Thomas Miner
Raymond B. Montgomery, Sc.D.
Ross F. Nigrelli, Ph.D.
Lars Onsager, Chem. E., Ph.D.
Charles E. Resser, Ph.D., Sc.D.
David McKenzie Rioch, M.D.
Robert Rugh, Ph.D.
Herbert J. Spinden, Ph.D.
Wendell M. Stanley, Ph.D.
Harold C. Urey, Ph.D., Sc.D.
Alexander Wetmore, Sc.D., Ph.D.
John C. Whitehorn, M.D.
Harold G. Wolff, M.D.

Honorary Life Membership was conferred upon the following eminent scientists:[1]

Arthur H. Compton, Ph.D., Sc.D., LL.D., Physics, Charles H. Swift Distinguished Service Professor, University of Chicago, Chicago, Illinois.

Clinton Joseph Davisson, Ph.D., Sc.D., Physics, Member Technical Staff, Bell Telephone Laboratories, New York, N. Y.

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[1] The class of Honorary Life Membership of The New York Academy of Sciences is limited to fifty.

After the Business Meeting, the following program was presented:

"The Mobilization of American Scientists for War"

by

FRANK BALDWIN JEWETT

"Direct Lift Aircraft"

(Motion Pictures in Color)

by

IGOR L. SIKORSKY

S. CHANDRASEKHAR, Yerkes Observatory, Williams Bay, Wisconsin: *New Methods in Stellar Dynamics.*[1] (A. Cressy Morrison Prize Winner, 1942.)

This paper presents a condensed version of certain new methods to investigate the dynamics of stellar systems. The statistics of the gravitational field are applied to the case of a *moving* body. It is shown how the upshot of the accidental fluctuations in the field produce a secular slowing down of bodies (stars) that have relatively high velocities, in this way retarding the escape, e.g. of stars from clusters. The rate of escape of stars from clusters is one of the leads used in evaluating the age of the universe.

CHARLES D. MICHENER, The American Museum of Natural History, New York, N. Y.: *Comparative External Anatomy, Phylogeny, and a Classification of the Bees.* (A. Cressy Morrison Prize Winner, 1942.)

It is the purpose of this paper (1) to add to the knowledge of general insect morphology and determine the homologies of certain previously unexplained structures, (2) to provide a uniform terminology for structures, (3) to discuss the comparative anatomy of bees and, with this as a basis, (4) to suggest the interrelationships of the various groups of bees and to provide a classification with keys to the families, subfamilies, and tribes.

In order to fulfill these objectives a detailed study was made of the skeletal morphology of *Anthophora*, a bee of intermediate specialization.

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[1] Abstracted by Professor Jan Shilt, Columbia University, New York, N. Y.

Wherever desirable, comparisons with other Hymenoptera or with insects of other orders were made in order to establish the homologies and the proper names for parts. Then numerous other bees, selected from all of the major subfamilies, were compraed with *Anthophora*. Finally, with these comparisons as a basis, a phylogenetic system and classification were devised.

The great majority of the modifications of *Anthophora* as contrasted with the lower Hymenoptera subserve but two functions, feeding and flight.

In the course of the work on comparative morphology, much evidence for parallel development of certain structures was obtained, and it is concluded that the most used classification of bees, based on the presence or absence of the pygidial plate, is unnatural, since this structure has been repeatedly lost in the course of evolution. A new phylogenetic tree is presented, based on numerous characters. The Paracolletini are regarded as the most primitive living bees. It is shown that with the exception of forms nesting in wood, most of the bees of Australia belong to primitive families.

The study of comparative anatomy and classification revealed numerous characters either ignored by previous students or not observed by them which are of utmost importance in determining the relationships of bees and even in identifying them. In the section on classification, keys are given to the families, subfamilies, and tribes of the world and to the genera of America north of Mexico. Six families, 22 subfamilies, and 32 tribes are recognized.

ARTHUR W. AND PRISCILLA F. POLLISTER, Department of Zoology, Columbia University, New York, N. Y.: *The Relation between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails.* (Awarded Honorable Mention, A. Cressy Morrison Prize Competition, 1942.)

The centrioles and chromosomes have been intensively studied in typical and atypical spermatogenesis of *Viviparus malleatus*; and brief confirmatory observations have been made on three other species of viviparid snails.

Typical spermatogenesis has no unusual features. The two nucleoli fuse when their associated chromosomes synapse. This fusion nucleolus disappears before the stage of diakinesis. The two centrioles of the first meiotic prophase take positions at the idiozomal pole of the nucleus in leptotene; and they remain there until they move apart to the spindle poles of the first division. The centrioles divide in the telophase of the first division; and one of the four thus formed is included in each of the four spermatids.

Atypical spermatocytes become much larger, due to more cytoplasmic growth. There is no synapsis. At a stage which seems, from the disappearance of the nucleolus, to be early diakinesis, the sister chromatids separate from one another. As a consequence, in late diakinesis the nucleus contains 36 (4N) separate chromatids instead of 9 (N) tetrads. Of these 36 chromatids four show approximately normal anaphasic behavior at the two meiotic divisions. The other 32 behave as if the mitotic orienting mechanism (the centromere, kinetochore, or spindle attachment point) were non-functional or absent. The 32 abnormal chromosomes degenerate, disappearing in the early spermatids.

In atypical spermatocytes the two centrioles grow to be large spheres of equal size, the centriospheres, during the spermatocyte growth period. There is evidence that the growth of the centriosphere is a result of the accretion of material that flows out of the nucleus at its idiozomal pole. At late diakinesis each centriosphere becomes resolved into a number of centrioles, which in size and staining character are like the two of normal spermatocytes of the typical line. Each of these centrioles divides in telophase I, at the normal time for centriole division. At prophase of the second division each centriole is surrounded by a small aster; and each centriole later goes to one pole of the division figure. In most cases in *Viviparus malleatus* one spermatid receives a larger number of centrioles. The atypical spermatozoon is multiflagellate because each centriole functions as a blepharoplast.

From a statistical study of the number of centrioles in the spermatids it is evident that each quartet of related spermatids (those descended from a single primary spermatocyte) contains 36 centrioles—an excess of 32 over the normal 4 centrioles. This number of extra centrioles agrees exactly with the number of chromosomes that behaved as if they lacked centromeres—as if they were, in Darlington's terminology, *acentric*. From this numerical comparison it is concluded that the 32 extra centrioles are actually the transformed centromeres of the 32 acentric chromatids; that of the total 36 centrioles there are 4 true centrioles (corresponding to the 4 of the typical line) and 32 centromeric centrioles. The centriole and chromosome counts in representatives of three other genera of viviparid snails, which have different chromosome numbers, likewise show that the number of extra centrioles always agrees exactly with the number of chromatids that behave as if acentric.

From the fact that all the centrioles of the atypical line behave like normal centrioles, and show no individual differences in function, it is concluded that centromere and centriole are essentially similar bodies, which normally show differences in behavior only because one is in the nucleus as a part of a chromosome while the other is in the cytoplasm.

Thus it is held that the extra centrioles in atypical spermatogenesis of viviparid snails do not arise, as has always been supposed, by repeated divisions of the two centrioles. Instead, the increased number of centrioles is a result of the contribution from the nucleus—in the form of centromeric substance. In the discussion it is pointed out that in a normal centriole cycle the centriole divides only after it has been in close connection with the centromere by the half-spindle fibre; and it is suggested that the division of the centriole is normally a consequence of its receiving an addition of centromeric substance flowing along the half spindle. The general view that centriolar material always originates from the nucleus is developed; and it is pointed out how this explains some of the hitherto puzzling centriolar phenomena—for example, the appearance of centrioles in the spermatogenous divisions of plants that lacked centrioles in the vegetative divisions.

T. Stanley Westoll, Department of Geology and Mineralogy, University of Aberdeen, Scotland: *The Haplolepidae, a New Family of Late Carboniferous Bony Fishes.* (Awarded Honorable Mention, A. Cressy Morrison Prize Competition, 1942.)

A group of late Carboniferous actinopterygian fishes is described and referred to the genera *Haplolepis* Miller, 1892 (syn. *Mecolepis* Newberry, 1856, *Eurylepis* Newberry, 1857—both preoccupied) and *Pyritocephalus* Fritsch, 1893 (syn. *Teleopterina* Berg., 1936). They are from six localities, of which the stratigraphical horizons can be stated by reference to Dix's succession of coal-measure floras.

In several cases it has been possible to make very detailed reconstructions of the dermal bones and the body, but remains of the endoskeleton are too bad for description.

The fishes are shown to be closely related and sharply marked off from all other known Carboniferous Actinopterygii: the new family, *Haplolepidae,* is set up for their reception. It is shown that the family was probably derived from the lower Carboniferous *Canobius,* which in turn is derived, through *Mesopoma,* from a series of "normal" palaeoniscoids allied to *Rhadinichthys.* The family was thus well separated from most Carboniferous evolutionary lines within the palaeoniscoids. It is shown that *Aeduella* Westoll is not a close relative of the Haplolepidae, and that the resemblances are largely due to parallel evolution; and that *Trissolepis* Fritsch, of which the skull-roof has been described as fenestrated in a manner similar to that of *Pyritocephalus,* is probably of normal palaeoniscoid structure with an entire skull-roof. The subgenera of *Haplolepis,* and *Pyritocephalus,* are separate throughout their known range, but *Pyritocephalus* was certainly derived from an early *Parahaplolepis.*

The comparative anatomy of the dermal bones, the body and the fins is discussed in detail. Peculiar modifications of the skull-roof and cheek, some of them independently undergone in different members of the group, are demonstrated. The development and significance of the paired fenestrations in the skull-roof of *Pyritocephalus* are discussed. Attention is given also to the remarkable modifications of the snout, external nares and opercular apparatus; to the presence of a flattened venter between the paired fins, associated with a modified course of the scale-rows of the body; to the frequent development of very deep scales on the anterior flank; to the modifications leading to the extreme thinness of the scales; to the laterosensory system, which is very fully developed and includes main and accessory, together with dorsal and ventral. lateral lines, the last described for the first time in an actinopterygian; and to the significance of the structure and nature of the dermal fin-rays, and of the relative positions of the fins.

Many of the characters demonstrated in Haplolepidae are to be found also in some members of the "Holostei" and "Sub-Holostei." A critical discussion of the status of these unnatural groups leads to a general statement of a new hypothesis of the evolution of the "holostean" condition, in which it is shown that the characteristic modification of the tail-fin has happened independently in different lineages, and is correlated with a great change in habitat and environment. This section is based largely on a consideration of the fish as a swimming organism.

The geographical distribution of the Haplolepidae is remarkably extensive for so specialized a family; in particular, the highly specialized *Pyritocephalus sculptus* from Bohemia is extremely similar in all details to the contemporaneous *P. lineatus* from Ohio. An analysis of the probable conditions of deposition of the rocks yielding the fossils leads to the conclusion that the Haplolepidae were fresh-water fishes thoroughly adapted to life in stagnant swamps, and almost certainly incapable of migration by sea. The implications of such conclusions in the understanding of the distribution and nature of coal-measure swamps, and on the theory of continental drift, are considered. The faunas associated with Haplolepidae are shown to include certain peculiar Amphibia, the distribution of which is extremely closely correlated with that of the fishes. The fate of this "facies fauna" is described, and the food relations of the various members are briefly noted.

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SECTION OF BIOLOGY

JANUARY 11, 1943

DOCTOR MYRON GORDON, Research Associate in Genetics, New York Zoological Society, New York City, N. Y.: *Genetic Studies of Speciation in the Swordtail-Platyfish Group and of the Experimentally Produced Hybrids.* (This lecture was illustrated by lantern slides.)

The purposes of this paper are twofold: first, to present a preliminary analysis of the processes of speciation in the Mexican swordtail-platyfish group as it is found in nature, in the rivers, brooks, springs, ponds and estuaries in Mexico and Guatemala; and second, to trace the methods by which some wild, quite uniform species become extremely variable under domestication, in the aquaria of fish-culturists.

In nature the species of *Xiphophorus* and *Platypoecilus* are restricted to the freshwater streams of the Atlantic drainage areas extending from northeastern Mexico to southeastern Guatemala. Extensive collections of the xiphophorin fishes were made on three personally conducted expeditions and these collections were augmented by material held in the various museums of this country. It is believed that most of the preserved specimens have been examined and recorded as to type.

The history of variation of the swordtail in the aquaria of fish-culturists has been traced from the literature. The task was not particularly difficult because swordtails and platyfish were imported as aquarium fishes only since 1907-1909 and new varieties were first reported in 1910. The details concerning the methods of breeding new varieties is rarely

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given, but this has been obtained by duplicating many of the varieties by crossing fishes from basic stocks of swordtails and platyfishes.

Representative samples of all the seven xiphophorin species were shipped alive to the laboratories of the New York Zoological Society for genetic analysis. Breeding tests have served several purposes. They made possible the evaluation of the genetic consistency of the taxonomic "key" characters which are used to distinguish the various subdivisions of the group. Breeding tests have also been applied to many minor characters such as distinctive markings which appear in some species but not in others. Many discrete patterns have been identified and have been proved to be inherited. Taken individually, these minor traits of coloring cannot be used in taxonomic procedures. Treated statistically, it has been found that some isolated populations of the same species can be distinguished by the presence or absence of some markings or by a particular frequency value for a particular pattern. The data on the distribution of patterns and their frequencies have been correlated with data generally used by the fish taxonomist, such as fin ray count and various body indices. When this has been done, great gaps in the distribution of some patterns have been found to coincide with the distinguishing taxonomic characters. Such studies in gene distribution add further to our information on the subject of microevolution, a phase of the revived study of the origin of species.

In a recent, but as yet unpublished, revision of the xiphophorin fishes by Carl L. Hubbs and the author, two genera are recognized, confirming Regan's 1913 interpretation of their relationship. The fishes are members of the Poeciliidae which in turn is one of the seven families of the teleost Order Cyprinodontes.

The genus *Xiphophorus* was established by Heckel in 1848 for *X. hellerii* and the genus *Platypoecilus* was designated by Gunther in 1866 for *P. maculatus*. The closeness of these two superficially unlike forms was not pointed out until 1913, at which time Regan demonstrated their relationship by his study of their gonopodia, the modified anal fins. Regan may have received a clue by the work of aquarists, who showed that hybrids between *X. hellerii* and *P. maculatus* were possible as early as 1911. Regan pointed out that the tip of the *Xiphophorus* gonopodium bore two terminal hooks, whereas *Platypoecilus* had but one. Other "key" characters separating the genera were the extension of the caudal fin in the male swordtail and the high number of dorsal fin rays. As more species of both genera were discovered the "key" characters in themselves were insufficient to clearly define the separation. Nevertheless, in a recent survey of the factors involved it has been decided to maintain the two genera for the time being.

On the basis of their geographical distribution, Mayr in 1942 suggested that the four members of *Platypoecilus* are but one species. Starting in the northernmost limit of their range and going toward the south, the following table indicates clearly, that no two species or subspecies of the platyfishes are found in the same river system or systems (based upon Hubbs and Gordon in MS.).

SPECIES	RIVER SYSTEM	DORSAL FIN RAY NUMBER
P. couchianus	Rio Grande	9.2
P. xiphidium	Rio Soto la Marina	10.0
P. variatus subsp.	Rio Panuco	11.0
P. variatus subsp.	Rio Cazones	11.6
P. variatus subsp.	Rio Tecolutla	12.2
P. maculatus	Rio Jamapa Rio Papaloapam Rio Coatzacoalcos Rio Usumacinta	9.8

P. couchianus is found only in a spring run at Santa Catarina and ten miles down stream in the Rio Santa Catarina at Monterrey, Neuvo Leon. The bulk of the population is restricted to the spring pool within the Huasteca Canyon and only a few specimens are found elsewhere; thus this species has a most restricted range, not over ten miles. With respect to color-pattern variation for which the species of platyfishes is well known, this species is the exception, for it has only a weakly spotted variety. Where their numbers are greatest in the spring run, no predatory fish species are associated with *P. couchianus*. Thus, the lack of variability may be correlated with its limited range.

P. xiphidium Hubbs and Gordon was taken in six rivers of the Soto la Marina system. This species has a number of striking patterns on or near the posterior tip of the caudal peduncle and the anterior region of the caudal fin: *M* for moon, *C* for crescent, *Ct* for cut crescent, and an overall irregular pattern called spots, *Sp*. Breeding tests indicate that these patterns are inherited either as autosomal or sex-linked genes.

By comparing the various populations of *P. xiphidium* from each of its six river populations it becomes clear that they are not genetically homogeneous, for in the more northern stream only a small percentage of the individuals have the spotted gene, whereas in the southern portion the percentage rises to over seventy. There are other small but significant differences with respect to the frequencies of the other patterns. Thus, in some cases, one could distinguish local populations on the basis of their gene frequencies.

P. variatus was taken over a large area. This species breaks up into a number of units, some of which have become so differentiated that it was thought desirable (Hubbs and Gordon) to set them off as subspecies. Even so, the unit remaining as *P. variatus variatus* is itself not entirely a homogeneous group considered on the basis of gene frequencies distributions.

Thus at the most northern position of its range, the population in the Rio Guayalejo may be distinguished by the fact that the spotted pattern, *Sp*, is represented in only 5 per cent of the total number of individuals. In the three populations of the Rio Panuco proper, the Rios Valles, Axtla and Tempoal, the *Sp* gene's frequency rises to 20 per cent. In the two estuaries, Cucharas and Tanchochin, the frequency falls again to 10.6 per cent. There is a transitional zone, represented by the relatively small river, the Tuxpan, which has a high percentage of the spotted type, over 40 per cent.

The *P. variatus* population in the Rio Cazones may be distinguished as a subspecies on one of the "key" characters, a high average number of dorsal fin rays, 11.6, instead of 11 for the others. The Cazones subspecies has an extremely high percentage of the *Sp* gene, just over 75 per cent.

The Rio Necaxa population of *P. variatus* (from the Rio Tecolutla system) has the highest average number of dorsal fin rays, 12.2, and upon this basis it was set off as another subspecies. This purely taxonomic conclusion is confirmed on the basis of the frequencies of *Sp* and other genes. The *Sp* gene frequency is high since over 65 per cent of the Necaxa population carries it; but the most important feature is that the Necaxa subspecies contains no *M*, *C* or *Ct* genes, the only population of *P. variatus* lacking these patterns. This form is particularly interesting since collections were made in widely different ecological conditions. For example, there is a thousand foot waterfall near the village of Necaxa, Puebla. Above the falls, specimens were taken from fast waters and from small quiet spring runs. Similarly below the falls, the fish were taken from torrential river conditions amid huge boulders where the water flowed so fast that footing was difficult; yet another collection was made in a small tributary with practically no flow of water, and still another collection was made in a spring pool. The taxonomic character, the high average number of dorsal fin rays, was in evidence as was the high frequency of the *Sp* gene and the absence of other color genes.

The Rio Nautla, south of the Tecolutla, has not been explored ichthyologically. This is unfortunate, since this is the region of break between *P. variatus* and *P. maculatus.*

The northernmost limit of *P. maculatus* is in the Rio Jamapa, at a point near the city of Vera Cruz. The same species is found in the major river systems all the way down to the Guatemala-Honduras border. No platyfish were found in the Rio Grijalva headwarters in Chiapas, but undoubtedly they occur in farther downstream portions, in Tabasco, since swordtails have been reported there.

P. maculatus is the most variable vertebrate in North America with respect to color patterns. One hundred and thirty-seven single patterns and pattern combinations have been observed which involve black markings alone. These may be separated on the basis of 16 units, some of which are inherited as autosomal, and others as sex-linked genes. In addition there are four kinds of red patterns which combine at random with all the others.

By comparing the pattern frequencies in each of the four great river systems from which *P. maculatus* is known, certain features become evident: the Rio Jamapa population is distinguished by the absence of the moon complete, *Mc*, and the single moon, *M*, patterns; the Rio Papaloapam population has all the patterns; the Rio Coatzacoalcos population has no single crescents, *C*, and no spotted sides, *Sp*, and an extremely high count of the black-sided pattern, *N*; the Guatemala, Rio Usumacinta population is the least variable group lacking the moon complete, *Mc*, the single crescent, *C*, the comet, *Co*, and the twin-spot, *T*.

We were fortunate in finding, in the various museums, collections of *P. maculatus* from the Rio Papaloapam, taken over a period of 75 years. One collection was made in 1867, another in 1902, another in 1932, and in 1939. By comparing the frequencies of patterns in fishes taken at these various periods, but from about the same general area, we can get an idea of the relative stability of the genes in time. With respect to the genes *Sp*, *O* and *Cc*, a remarkably uniform distribution is found, indicating as Dr. Mayr, 1942, has pointed out that alternative patterns, or polymorphic patterns, are probably neutral in reference to the forces of selection. This is borne out, too, by the data collected by examining specimens of *P. maculatus* at exactly the same spot over a period of six important days in the history of one population. In the period just prior to the complete drying up of a pool, the water becomes quite shallow, permitting predatory birds to wade in and pick out fishes with great ease. The fish population becomes concentrated in a small area. All species of fish are subject to predation, including predatory species of fishes. Pattern frequencies were studied from samples taken on March 4, 6, 7 and 10. No significant differences were found in proportion of patterns remaining. The size of the fish was the only definite factor, the largest going first. For example, the predacious fishes, like *Belonesox*, which are three or four times as large as *Platypoecilus*, were removed by March 6 and 7. None was found on March 10. Toward the end of the period only tiny representatives, chiefly of the platyfish and *Gambusia* and tiny characins, remained; nearly all the remaining fish were less than one inch long.

The general conclusion which may be drawn from the data on *P. maculatus* is this: subspeciation has not yet been completed, but the beginnings of the process are reflected in the genetically different isolated populations in the four large river systems.

Factors other than geographical isolation have been at work in the genus *Xiphophorus*, since two species, the montezumae and the pygmy (Hubbs and Gordon, Copeia, 1943, No. 1), are found in the same river, at least in the same cross section of the Rio Axtla, San Luis Potosi. The pygmy is found in the rapidly flowing, deep water that runs along the cut-in slope, whereas *X. montezumae* is found in quite shallow water of the slip-off slope. Only two specimens of the pygmy have found outside fast, deep water and no *X. montezumae* has been taken in the typical pygmy's habitat. The range of the pygmy is as limited in space as *P. couchianus* and it too is a relatively uniform species.

The gonopodium of the pygmy is of the *Xiphophorus hellerii* type whereas that of *X. montezumae* appears to be intermediate between the typical swordtail and a typical platyfish. It may be suggested that *X. montezumae* is a natural hybrid between the pygmy swordtail and *P. variatus variatus* which also inhabits the Rio Axtla area. At two collecting stations the three species were taken together. In over 2000 specimens taken none could be identified as a hybrid and as yet none has been hybridized in the laboratory.

In the Rio Axtla where the three species of xiphophorins appear together each species has a distinctive group of markings. *P. variatus* has 17 per cent with crescents, *C*, and 12 per cent with the cut crescent, *Ct*. *X. montezumae* does not have these patterns and neither does the pygmy. The former two have the spotted, *Sp*, and the single moon, *M*. Thus if *X. montezumae* is a hybrid, it should have the crescent or cut crescent markings, but it does not.

The typical habitat of *X. montezumae* differs from that of *P. variatus* even though, occasionally, they are found together. *X. montezumae* is found in more upland streams, in cleaner, faster waters. In many such localities the swordtail is the only representative found. On the other hand, the platyfish is found more often nearer the coast, and in more stagnant water. Often in such a situation it is the only representative found.

In the course of time many species of *Xiphophorus* have been described from Jalapa, Vera Cruz, south to Guatemala but they are now all referred to *X. hellerii*. This species was recognized in 1848 by Heckel from a collection made by Heller near Cordova, Vera Cruz. Hubbs and Gordon tentatively have employed the former species names to represent what they regard as subspecies in the many racial stocks of this species. The taxonomic characters, the number of dorsal fin rays, are not consistent and the separation is based upon the increasing depth of body in fish of various localities. The use of pattern genes cannot help in solving this complex problem of subspeciation since the species as a whole lack definitive markings. There is a general change in appearance but this has not as yet been analyzed genetically.

At many points, the habitat of *X. hellerii* and *P. maculatus* are identical, yet on the basis of an examination of over 10,000 specimens, not a single hybrid between these species has been found in nature. It would have been easy to detect such hybrids, if they existed, on the basis of their modified color patterns. Under aquarium conditions these species hybridize readily. The failure to do so in nature may be explained probably in certain subtle but important differences in breeding behavior pattern. Mating between them may, under special conditions, take place in nature but foreign sperm may be at a disadvantage in the ovarian fluid of the different species. Under laboratory conditions *X. hellerii* females do not produce young hybrids at periodic intervals, after the males have been removed, as they are likely to do when mated with their own kind.

In light of the uniformity of the wild *X. hellerii* with respect to the many color variations possessed by its neighbor *P. maculatus*, it is remarkable that domesticated swordtails are so extremely variable. There are ten or more distinctive aquarium-reared varieties of swordtails. The history of each type is shrouded in mystery, for rarely has the fish culturist revealed his methods of breeding new types of swordtails. Most of the domesticated swordtail varieties came into being owing to chance hybridization with *P. maculatus.* Only the albino swordtail can be attributed to mutation effect; the others owe their origin to gene transfer. Swordtails have appeared with one-spots, twin-spots, crescents, moon, comets, black spots, black bands, orange, red and golden. One swordtail variety originating in 1911 was falsely called *X. montezumae* because of its unusual pattern, being brassy red and having several rows of black spots. Only recently has this false montezuma been resynthesized from pure *P. maculatus* and pure *X. hellerii.* The factors involved were the dorsal red, *Dr*, and the striped pattern, *Sr.* The first generation is generally crossed back to the swordtail which gives the young of the next generation the appearance of a swordtail but with the modified pattern of *P. maculatus.*

A remarkable transformation takes place in the phenotypic expression of various *P. maculatus* genes when transferred to *X. hellerii.* For instance the spotted factor, *Sp*, in *P. maculatus* is responsible for a lightly marked pattern brought about by the scattered macromelanophores. This gene, *Sp*, in association with the modifying factors of *X. hellerii*, brings about a rapid multiplication of macromelanophores in the hybrid, so that the hybrid becomes melanotic and in some individuals a true neoplasm, a melanoma, develops as a direct result of this combination of foreign genes. The spotted dorsal fin gene, *Sd*, in the hybrid with *X. hellerii* produces a melanoma of the dorsal fin. The *N* factor produces a tumor of the flanks.

The modified expression of the red factors, *R*, *Rt*, *Dr*, of *P. maculatus* may also be seen in hybrids with *X. hellerii.* The coloring becomes darker and more intense with every backcross to the swordtail. No tumors develop as a result of this modification.

On the basis of a survey of the color patterns of the wild *P. maculatus* and the gold variety which appeared as a mutant in the aquarium, most domesticated varieties of the swordtail may be derived from the fundamental patterns of the platyfish. This will be demonstrated in a forthcoming publication. The swordtail varieties as now seen in aquaria are of hybrid origin through a process of gene transfer. Gene transfer may also account for the increased brilliance of some of the *P. maculatus* domesticated types, for these have some *X. hellerii* genes.

Sometimes special genetic technique must be applied to show the presence of swordtail genes in unadorned *P. maculatus* of the domesticated type. First, it has been shown that when the wild variety comet platyfish is mated with the wild swordtail, the hybrids develop the wagtail pattern, one in which all the fins of the body become deeply black; whereas the comet pattern in the wild platyfish is marked only by a black line on the upper and on the lower margins of the tail. When the comet of the wild platyfish is mated with a domesticated unadorned goldplaty, this wagtail reaction reappears. In explanation, it may be said that the goldplaty, a domesticated variety, had the specific modifier of comet and it obtained this modifier from a hybrid ancestor. A similar case may be made out for the brilliant domesticated types of the red platy. Light red forms of *P. maculatus* may be found in nature. The light red coloring becomes intensified in the hybrid. By selective matings the brilliant coloring may be transferred to a form indistinguishable from the type of *P. maculatus*. Thus the origin of the domesticated platyfish and swordtails is polyphylletic.

Collections of material for this paper were made by three Mexican expeditions. The first, in 1930, was sponsored by the University of Michigan and the National Research Council Fellowship; the second, in 1932, was sponsored by Cornell University through the August Heckscher Research Foundation; and the third, in 1939, by the John Simon Guggenheim Memorial Foundation Fellowship. The author wishes also to acknowledge aid by the Department of Birds of The American Museum of Natural History for the use of their laboratories in the Whitney Wing and by the New York Zoological Society for use of space in the old New York Aquarium at Battery Park.

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SECTION OF PSYCHOLOGY

JANUARY 18, 1943

MR. GREGORY BATESON, Film Library, Museum of Modern Art, New York, N. Y.: *Cultural and Thematic Analysis of Fictional Films.* (Illustrated by motion pictures.)

This paper is a preliminary report on a piece of research now in progress at the Museum of Modern Art Film Library. The purpose of this research is to derive some notion of the psychological implications of Nazism from the study of Nazi propaganda films. This involves the application of anthropological methods of analysis to a variety of German cultural materials—the analysis of film material provided by the Film Library and of verbatim interviews of Germans and social scientists familiar with German conditions conducted under the auspices of the Council on Inter-cultural Relations.

Anthropological analysis consists always of two parts. First, the recognition of significant themes and second, the verification that these themes are in fact characteristic of the culture that we are studying. In this research the themes are exemplified in the films and the verification has been done by interview techniques.[*] As usual in such research, the peculiarities of daily life and daydream are referred back to the family setting and especially to the position of the child in that setting. This procedure can be justified by the fact that the most formative years of an individual's life are spent in that setting, but the practice of referring the remainder of a culture to the family background is also necessary on practical grounds. It enables us to compare one culture with another. All societies have the institution of the family, but not all societies have Hinduism or age grades. Thus, by referring the peculiarities of Hinduism to the family structure, instead of *vice versa,* we arrive at systematic statements which are comparable from one culture to the next.

This method of referring cultural peculiarities to the family is specially suitable for the analysis of Nazi propaganda. In America we tend to think of propaganda as consisting of a large number of separate utterances, pious sentiments or jokes, inserted into the more or less propagandically neutral matrix of communication. Publicity methods were developed on the basis of rather simple psychological theories of association and Watsonian conditioning and have been comparatively little influenced by *Gestalt* Psychology or psychoanalysis. The significant propaganda in the German films is, however, not of this sort. It consists not of isolated utterances but of themes built into the structure of the plot in such a way that the audience, while enjoying the plot, will necessarily accept the underlying themes as basic premises which need never be articulately stated. The underlying themes, which are expertly woven into a background for the promotion of Nazism, are the themes of pre-Nazi German family life—are, in fact, the themes which cultural analysis is best equipped to recognise.

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[*] The writer was intimately connected with work done by E. H. Erikson and since published. ("Hitler's Imagery and German Youth," by Erik Homburger Erikson. Psychiatry, 1942, Vol. 5 pp. 475–493.) The present research supports a number of Erikson's conclusions based on other types of German material. Acknowledgment must also be made to The American Museum of Natural History for the use of facilities.

This use of the family is characteristic of the Nazi film, "*Hitlerjunge Quex,*" made by Ufa and released in September, 1933.*

The hero of this film is Heini, a preadolescent boy, the son of a violent father and a drudge mother. His parents are of lower middle class and have fallen in the world as a result of the inflation and the father's war wounds. The father hurts himself in a minor food riot which results from the stealing of apples by two hungry Communist boys. He is helped home by Stoppel, an organizer of Communist youth. He lives in a poor Communist district of Berlin. Stoppel helps the mother to dress the father's wound. The father asks the mother for money to get some beer. She says that she has no money, and a violent scene follows, the father ransacking all the containers in the house in search of the mother's hiding place. At this moment Heini returns from his work in a printing shop, arriving so that we see the climax of the scene between the parents through the eyes of the son. At the printing shop Heini had received a tip of one mark. He secretly gives the mark to his mother. She gives it to the father, who then goes off to get his beer. Stoppel is impressed by Heini's character and contacts him with a view to enlisting him in the Communist Youth.

After Stoppel has left, the mother opens the window and lets in the music of the merry-go-round at the fair. Heini is thus reminded of a wonderful knife which he has seen in a lottery in one of the side shows. He asks his mother for money to enter this lottery, which he is sure that he will win. She gets money from her hiding place in the coffee grinder and gives it to him. Heini goes to the lottery and loses.

Stoppel appears at Heini's elbow, comforts him and invites him to join a hike of the Communist Youth on the following day. Heini accepts.

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* The first three reels of this nine-reel film were shown at the meeting, by courtesy of the Film Library.

On the hike Heini sees the Hitler Youth, a company of whom are on the same train, going hiking to the same woods. He is disgusted by the gross behavior of the Communists and especially by a kiss which Gerda, one of the Communist girls, forces upon him. Finally he wanders in the dark, away from the Communist camp, till he hears the Song of the Hitler Youth coming from the Nazi camp. He gazes through the bushes at the Nazis who are celebrating the Summer Solstice. The Nazis find him, accuse him of spying and send him away. He sleeps by himself on the ground and next morning after watching the Nazis with longing eyes he goes home to his mother.

He tells her he was with the Nazis and how wonderful they were. His mother is worried but not angry with him. She even lets him sing the Nazi Youth song (without warning him that the father is in the next room). The father hears the song and comes in furious. He compels Heini to sing the "Internazionale," boxing his ears while he sings.

Next day at school Heini again meets Gerda and resists her advances. He approaches Fritz, a boy leader of the Nazis, and is invited to supper with Fritz and Fritz's sister, Ulla. Gerda meanwhile has been told by Stoppel to vamp Grundler, a weak Nazi boy, and while Fritz and Heini go off to supper, Gerda and Grundler go off to seek "Turkish Delight" at the fair.

Fritz and Ulla ask Heini to come to the opening of their new Nazi club room (*"Heim"*). Heini hesitates because he has no key with which to return home after the meeting. Finally he accepts when put on his mettle by Fritz.

The father meanwhile has been persuaded by Stoppel to sign Heini into the Communist Youth. When Heini comes home the father at once informs him that he is to go that night to the Communist Local. Heini says, "Do I get a house key?" The father says, "Of course—you are now a grown man, and a grown man has a house key." The father then makes a long friendly speech to Heini about the difficulties of life and how "you young ones must help us older people." Heini is almost in tears and says later to his mother, "Father is not so bad—I cannot lie to him—he gave me a house key." He is determined, however, not to go to the Communist meeting.

He goes out that evening but meets Stoppel, who draws him aside into a doorway and tells him that the Communists are going to raid the

Nazi home and that he is to help in the raid. Heini manages to slip away without taking an active part, but when the police come he is picked up.

The police tell him to "go home to Mother." The Nazis think he has set the Communists onto them and accuse him of treachery.

(We are given no further information about the events of that night, nor do we see Heini use the house key to return to the bosom of his family.)

Next day Stoppel tells Heini that he was a hero because he did not tell anything to the police and that tonight they will get dynamite from the cache in the Marschstrasse and blow up that nest of Nazis. Heini protests and finally says he will warn them. Stoppel is very much shocked and tells Heini that that is something which one does "only once in life" but in the end Stoppel shrugs his shoulders and dismisses the matter.

When Stoppel leaves, Heini rushes to the telephone to warn Fritz and Ulla. Ulla answers the telephone but Fritz tells her not to listen to that traitor. Ulla is worried by Heini's mention of dynamite but she obediently hangs up, and Heini is left talking into a dead phone. In despair he tries to persuade the police to interfere but they treat him as a child. He then tries to find Stoppel but cannot.

Suddenly while Heini is in the fair looking for Stoppel there is a violent explosion—the Nazis have blown up the dynamite—and Heini returns home whistling the Youth Song.

His mother is in a state of despair because Heini has betrayed the Communists and she tries to persuade him to make up with Stoppel. She fails, and Heini goes to bed while the mother sits weeping. Finally she turns on the gas to kill both herself and Heini, and the screen is filled with the fumes (which billow like a flag).

Heini awakes in the hospital. A nurse says, "There is somebody to see you." Heini says, "My mother?" But it is the Nazi boys and Ulla. They give him a uniform and a mirror in which to admire himself. After they have gone the nurse comes; and while she is removing the Nazi cap from his head she tells him that his mother will never come.

While Heini is convalescing, his father and the District Leader of the Nazis happen to visit him simultaneously, and the question is discussed—"Where does the boy belong?" The Nazi wins this discussion by a verbal trick, and Heini goes to live in a Nazi clubhouse outside the Communist district.

The Communists are waiting for vengeance, but in spite of the District Leader's opposition Heini wants to return to the Communist district to distribute leaflets for the 1933 election. Grundler has been falling lower and lower under Gerda's influence and now he and Gerda destroy all the available Nazi leaflets. Fritz has been wounded in an election riot so that Heini and Ulla are brought together by substituting Heini for Ulla's brother. They work together in the printing shop to prepare new leaflets and when the work is completed Ulla gives Heini a sisterly kiss. Heini then goes to distribute the leaflets in the Communist district. He is hunted and encircled by the Communists in the darkened streets and takes refuge in one of the tents in the deserted fair.

Accidently he touches a mechanical figure of a drummer and the figure starts to beat its drum, thus betraying him. Heini is stabbed (presumably by Wilde, the sinister leader of the Communists, who has the original knife which Heini coveted). The Nazis come and find Heini dying. His last words are, "Our flag billows before . . . ." The sound track takes up the Youth Song and the flag appears on the screen, giving place to marching columns of Hitler Youth.

In this plot Heini's conversion to Nazism depends essentially upon the contrast which he is shown between the Nazis' picture of themselves and the Nazis' picture of "Communism." But this is not the only message which the propagandist conveys. At the beginning of the film the propagandist seems deliberately to build up an association between the mother and Communism. It is the mother who goes and opens the window and lets in the degenerate music of the fair, and the fair is the setting in which the Communists are most at home. And it is the mother who gives Heini the money to enter the lottery to try to win a knife from this depraved environment. Stoppel, the Communist organizer, is also associated with the mother, joining her in aiding and placating the father. In this way the audience is encouraged to accept unwittingly, the basic premise that ideology is related to the family structure.

Also at the beginning of the film we are shown Heini as a hero rescuing his mother from the father's violence by self-sacrifice. As the film progresses, we see these self-sacrificing attitudes shifted from the mother to the nation and the position of Communism shifted from its association with the mother to a very much more dramatic association with the father. The basic premise, that ideology is connected with the family structure, is allowed to persist, but the straw-man association between Communism and the mother is smashed when the father compels Heini to sing the "Internazionale," boxing his ears in time with the song. In this way the propagandist confers on Nazism not merely the virtue of Heini's preference for it, but also the whole fanatical gamut of emotions which are evoked in the Oedipus situation.

The contrast presented between Nazism and Communism on the hike is interesting in that it shows another aspect of Nazi psychology. One of the basic premises of the film which is nowhere articulately expressed is that Communism and Nazism are psychologically related. We see, for example, the behavior of the Nazi boys at the railroad station when one of the Communists throws chewed food into the face of one of the Nazis. Their instinct is to break their ranks and to lapse into disorderly aggression. Similarly, in Grundler, the weak Nazi, we see how normal heterosexual temptation may undermine the Nazi character until he becomes depraved like "Communists." The assumption is that without discipline pure Nazis degenerate into the picture which they themselves have drawn of "Communism." In other words, this particular picture of "Communism" has no factual relationship to the real thing but is a self-portrait of Nazism—a portrait of what the Nazis think that they themselves are like under the veneer of discipline.

From this point of view it is interesting to observe the large number of oral and anal characteristics which the film maker showers upon "Communism." The apple which fills the first frame and which is there sympathetically treated—it is a desirable apple, stolen by the Communist boy under stress of hunger—is a symbol, whose oral and sexual implications are fully worked out. Continually we see the "Communists" indulging their mouths, eating coarsely, Gerda kissing Heini, and Stoppel pushing a banana into Gerda's mouth.

This curious double formation in Nazi character is likely to be of considerable importance after the war and it might almost be said that the closing of this split in the Nazi personality—between the over-pure and the over-dirty—will be essential for the stabilization of Europe. The split is not, however, merely of Nazi origin. It is older than that and is perfectly recognizable in German films of the early twenties (e.g. "The Street," "Variety," "M") and we are probably justified in regarding as an expression of the same split, the curious behavior of German troops at the end of the last war who found time during their retreat to soil their billets. In fact the problem of Germany is in part a problem of preventing a pendulum from swinging too far into aggressive purity in good times and into degenerate self-contempt in bad. And the extreme high point reached by this pendulum during the heyday of Nazism probably denotes an increased capacity for a later fall.

As we have seen, the lines for such a fall are clearly laid down in this film of 1933.

We cannot here examine in detail the methods of treating this split but, one incident in the film is suggestive. The film as a whole implies that Nazism is the total destruction of the family. In order to create a violent emotional adherance to Nazism the family itself is unscrupulously sacrificed. The woman's place may be in the home but she need not expect that home to contain a husband or children over six. These others, the men and the boys and the unmarried girls, will be absorbed into "Youth" organizations which free them from accepting the responsibilities of adult human status. The one feature of the film which appears discrepant with this treatment is the propagandist's implicit and probably unconscious confession that there is another way. The symbol of adult human status, which Heini is not allowed to use, is the house key,—a symbol which while conferring freedom also confers the promise of return to the family.

SECTION OF ANTHROPOLOGY

JANUARY 25, 1943

DOCTOR ERICH FROMM, New School for Social Research, New York, N. Y.: *On the Problems of German Characterology.*

The importance of the problem of national character at the present time is obvious. If we wish to wage psychological warfare, we must know something about the character of our enemies. For instance, how they react to bombing and casualties; whether they are more impressed by propaganda that speaks in threatening, harsh language or by one that speaks in reasonable language, and also whether one part of the enemy population may react differently from another part to each kind of propaganda.

For our peace plans, the important question is whether the Germans are a nation of sadists eager to conquer the world or whether these characteristics are true only of a minority of the German population which now forms the nucleus of the Nazi regime. This paper does not try to answer the pressing question of what the German national character is, but wants only to make some tentative suggestions with regard to the method of approach to the problem of national character.

So far, mainly anthropology and psychoanalysis have dealt with the problem of the national character. Anthropology attacks the problem by examining the spirit and the psychological atmosphere of various peoples. The contribution of psychoanalysis to the problem of the national character has been mainly to lay the foundations for the understanding of character structure.

The main principles of psychoanalytic characterology are:

1.—The structural viewpoint: Certain attitudes and character traits are not accidentally but necessarily connected, because they result from a basic form of relatedness to the world and oneself. Psychoanalytic characterology deals with syndromes of character traits.

2.—The dynamic concept of character: Character traits are particular forms in which human energy is chanelled. They are not habits acquired by training and easily changed by other training, but behind the character trait we find human energy and passion which makes it imperative for man to act according to his character. While psychoanalytic experience showed that aside from constitutional factors, the character structure is formed by the experiences of the child and later environmental experiences, it could confirm also Heraclitus' statement: "Man's character is his destiny."

3.—Environmental conditioning of character structure: It is important to note that there are reasons to assume that those conditions that make for different personality development differ, while those that make for the neurotic outcome of each personality structure are more or less the same—early intimidation, thwarting of growth, isolation, and especially one condition which one finds in many severe neuroses, that parents pretend to love the child while actually they dislike it or are indifferent to it. The child experiences something emotionally which it cannot cope with by his reasoning power and is confused and even more helpless than it might be by clearcut open intimidation.

The method of the older psychoanalytic writings in applying psychoanalytic characterology to social groups suffered from one main error. Instead of analyzing the minute details of the history of the group under examination, they drew conclusions by analogy, assuming that character traits in a group which were similar to those one had found in a patient were identical and could be explained by analogy. At the same time, these older writings leaned over backward in emphasizing the role of psychological factors in history by assuming that political and economic factors in themselves had little causative effect; these factors were therefore not sufficiently studied.

A recent paper by Dr. Brickner in the Journal of Orthopsychiatry on "The German Cultural Paranoid Trend" avoids some of the main mistakes of the older psychoanalytic method, while it repeats those with regard to examining insufficiently the role of economic and political factors, and especially, the psychological difference between different social strata of the German population.

While anthropologists have shown the fallacy of explaining differences in national character in terms of biological differences, psychoanalytic characterology can show that the differences we find are amply explained by the interaction of character, social and economic conditions, and ideas. At any point of the history of a nation where we start the investigation, we find certain characterological attitudes present and a specific mode of life which fits in with these character traits. In following up the historical development, we find that certain ideas and ideologies arise from this character structure and thus systematize and reinforce it; thus rationalizations for the desires specific to that character are given; in various ways certain desires (either in reality or in fantasy) are satisfied and means to cope with certain fears are provided. The character structure thus reinforced by ideas and ideologies tends to shape external conditions so that they fit in with the needs and potentialities of that particular kind of character structure and thus reinforce it in turn.

The family structure—character of the parents and methods of child rearing—transmits the atmosphere of the social structure to the child. Any given society creates the kind of national character the needs of which it can satisfy. This makes for the persistency of certain traits in the national character throughout generations. However, contradictions within the social structure make not only for socio-economic but also for certain psychological changes.

The question which is particularly relevant in present-day discussion is whether the national character of different nations differs in the sense that some are "good" and others "bad." The suggestion is made that one might differentiate between a "character matrix" typical of each nation and the positive and negative outcome which results from each character matrix respectively. It is assumed that according to different historical conditions, each nation develops certain basic character traits which, although not eternal, last through many generations due to the operation and interaction of the different factors mentioned before; that this relatively constant character matrix is indifferent with regard to value; that under certain conditions it develops into positive qualities, under others into negative ones. Nations are not different in the sense that some have vices and others virtues, but that each nation has its specific vices and virtues which are the outcome of its specific character matrix. As an example, two character traits which seem to be typical of the Germans as a whole are mentioned. One character trait may be called orderliness. In its positive outcome, it results in ability for organization, discipline and responsibility; in its negative outcome it results in pettiness, rigidity, sadistic and masochistic trends. Another characteristic seems to be a certain tendency for the kind of thinking which transcends empirically given data. In its positive aspects, it leads to the ability for visionary construction as shown in the great philosophical systems German philosophers have created or possibly in certain musical creations. In its negative aspect, it leads to the fantastic, satanic and unrealistic thinking of the Nazis.

Certain character traits of the Jews can also be explained in terms of the positive and negative outcome of their character matrix.

Much more study on the problem of character structure in general, and of the German character specifically, has to be done before the concept of the character matrix and its positive and negative outcomes can be more than a hypothesis. There are also some reasons for the following assumption: that the character matrix is due to different historical conditions, while the positive or negative outcome of each character matrix depends more or less on the same social and cultural conditions. The thwarting of life, manipulation, intimidation, any condition which hampers growth make for the negative outcome. Freedom, spontaneity and growth, conditions which take every individual as an end in itself and not as a means for the sake of others, make for the positive outcome.

One can find some evidence for this hypothesis in the German development. There is evidence to assume that the vicious side of the German character matrix was mainly exhibited by the lower middle class which formed the nucleus of the Nazi Party. This was a social group economically decaying; having lost the vestiges of social prestige it once had; being faced with the collapse of the authorities of the Monarchy with which it had identified itself; having lost the little money it had saved for many years during the period of inflation and being in a position where their sons had little chance to continue in the business of their fathers, but had to try to remain "respectable" members of the middle class by turning to the professions, thus confronting a highly competitive situation for which they were often not particularly well qualified. The self-esteem of this class was constantly hurt by the fact that it was their pride to belong to the middle class and yet that they were at the lowest level of this class.

On the other hand, there is evidence to assume that the German working class, by and large, did not exhibit the same traits of sado-masochism of the lower middle class. They were part of the rising industrial system, constantly improved their positions, developed a certain feeling of solidarity in contrast to the bitter individual competition typical of the lower middle class; and by not aspiring to be part of the middle class, were not hurt in their self-esteem. Certain facts support the hypothesis that the sado-masochistic traits characteristic of the lower middle class and the Nazis are not characteristic of the majority of the German population. One instance mentioned is the leniency with which the workers dealt with their political opponents in the brief, chaotic period of the German revolution of 1918 when the authority of their former political opponents had collapsed. Another instance is the necessity for the Nazi terror since the beginning of their power, which implies that the Nazis have not succeeded in imbuing the majority of the German population with their spirit to such an extent that they can dispose of terroristic tactics. Another instance is the reaction reported by many observers of the average German to the announcement of victories in this war. The reaction seems to have been one of indifference rather than of enthusiasm and excitement one would expect if the majority of the Germans were characterized by a deep-rooted lust for conquest.

A study is mentioned by Ernest G. Schachtel and Erich Fromm which examined the character structure of the German workers and employees in 1929. This study shows that not more than about 10 per cent of the subjects showed a character structure which can be called authoritarian (although not necessarily Fascist in terms of sadism), to whom it might be expected that Fascist ideology would appeal.

The paper comes to this conclusion:

The Germans, like any other nation, have a character matrix which is the result of a centuries-old interaction of character, ideas, social and political conditions. This basic complex in itself is neither good nor bad. It has its specific virtues and vices which are determined by certain conditions which differ from class to class. It happens that in Germany the lower middle class was the exponent of the vices, being exposed to the most unfavorable conditions.

While the Nazi functionaries are hopeless and will be destroyed by the Germans themselves once Hitler's power starts to crack, the conditions for the positive development of the Germans as a whole are exactly the same as for any nation or any individual: social conditions which make for freedom, solidarity, and growth. Under such conditions, the majority of the Germans will not lose their specific national characteristics but the positive side of these characteristics will be reinforced. The first political condition is the complete destruction of the Nazis and of those who are or have been interested in the rule of the Nazis. No compromise with them is permitted if the rest of the Germans are to develop the positive side of their character matrix.

NEW MEMBERS

ELECTED JANUARY 28, 1943

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Dewey, Jane Mary, Ph.D., Physicist, U. S. Rubber Company, General Laboratories, Passaic, N. J.

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Hershkowitz, Harry, M.S., Botany, Forestry, New York Police Department, New York, N. Y.

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Morse, Edna C., M.A., Chemistry, Instructor in Chemistry, Dept. of Home Economics, Columbia University, New York, N. Y.

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Salzmann, J. A., D.D.S., Orthodontics, Dentistry, Head of Dental Service, New York City Vocational Schools; Lecturer, Preventive Orthodontics, Guggenheim Dental Clinics, New York, N. Y.

Simha, Robert, Ph.D., Chemistry and Physics, Assistant Professor, Howard University, Washington, D. C.

Spilman, Carl Jonathan, Ph.D., Psychology, Richmond Hill, L. I., N. Y.

Tendick, Katharina Lois, B.A., Psychology and Social Sciences, Employment Counselor, Central Employment Bureau, Y. W. C. A., Brooklyn, N. Y.

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Charles, Donald R., Ph.D., Genetics and Comparative Anatomy, Assistant Professor of Zoology, University of Rochester, Rochester, N. Y.

Elias, Maxim K., Ph.D., Paleobotany, Invertebrate Paleontology, Associate Professor in Research, University of Nebraska; Paleontologist, Nebraska Geological Survey, Lincoln, Neb.

Hovorka, Frank, Ph.D., Physical Chemistry, Professor of Chemistry and Director of the University, Western Reserve University, Cleveland, Ohio.

Smith, A. F., Ph.D., Chemistry, Group Leader of Research Chemists, Du Pont Experimental Station, Wilmington, Del.

Sun, Kuan Han, Ph.D., Physical Chemistry, Research Chemist, Research Laboratories, Eastman Kodak Company, Rochester, N. Y.

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SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 1, 1943

DOCTOR DORR C. SKEELS, Geophysicist, Standard Oil Company of New Jersey, New York, N. Y.: *Gravity Anomalies in Sedimentary Basins.* (This lecture was illustrated by lantern slides.)

Although geodesists have studied the distribution of the gravitational force since 1740, it is only in recent years that gravity surveys have been made for geological purposes. These later surveys have been made chiefly by oil companies, and the data are not at this time available to the public. The geodetic pendulum data are available, however, but many of these results have never been analyzed from the geological standpoint. It is the purpose of this paper to call attention to some of these data, and to point out their bearing on one of the major problems of geology, the process of basin development.

BLACK HILLS–BIGHORN–BEARTOOTH PROFILE.—There is a close resemblance between the gravity profile and the geologic profile on the top of the crystalline basement. Each of the uplifts is a gravity "high," and the intravening basins are gravity "low." The Bouguer anomaly on the Black Hills uplift has a value of −75 milligals; it falls to about −170 in the western part of the Powder River basin, and then rises to −115 on the Bighorn block. It falls to −190 in the Bighorn basin, and rises to −170 on the Beartooth uplift. Thus the whole profile trends

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downward to the west; this regional effect is undoubtedly isostatic, and indicates regional compensation for the Rocky Mountain system as a whole, rather than individual compensation for each block. If a regional isostatic correction is made, then the Powder River basin becomes a minimum of 75 milligals amplitude; the Bighorn basin a minimum of 50 milligals.

ARDMORE BASIN, SOUTHERN OKLAHOMA.—This profile crosses the Ardmore basin, the Criner Hills uplift, the Marietta syncline, and the Muenster arch. The Ardmore basin is 75 milligals lower than the Arbuckle Mountains, and 45 milligals lower then the Muenster arch, after correction for isostatic effects. The Criner Hills anticline forms a high of 20 milligals amplitude between the Ardmore low and the low over the Marietta syncline. Geological data indicate at least 40,000 feet of sediments in the Ardmore trough.

PO BASIN, NORTHERN ITALY.—This profile shows a very pronounced low whose axis runs along the northern front of the Apennines. From a value of +50, in the neighborhood of Pisa, gravity decreases northward to a minimum value of −160 at the northern foot of the Apennines, then increases northward to a maximum value of +40 milligals in the vicinity of the Italian lakes. The gravity data suggest that the Apennines have been thrust over the southern flank of a very deep basin of Tertiary sediments, the northern flank of which is exposed in the Po valley. The amplitude of the anomaly is 200 milligals.

CARPATHIAN FORELAND, POLAND.—This profile crosses the Carpathian Mountains and the Miocene basin to the east, in the vicinity of Boryslaw. Relations similar to those in the Po basin are seen. The axis of the gravity minima follows the overthrust at the foot of the Carpathians. The inference is that the Carpathians have been thrust a distance of 40 kilometers or more over the Miocene basin.

CAUCASUS REGION.—Gravity minima flank the Caucasus Mountains, and occupy the area of the Tertiary sediments. The lowest value of gravity is near Baku (isostatic anomaly −120; Bouguer anomaly −140 mg.), where Goubkin estimates the depth to the crystalline basement as at least 12,000 meters.

KOUSNETSK COAL BASIN, U.S.S.R.—The axis of the gravity low lies near the southwest margin of the basin. The sediments in the basin range in age from Cambrian to Jurassic. As compared with the Tertiary basins previously discussed, the gravity minimum is only moderate; from a minimum value of −40 milligals (isostatic anomaly) the anomaly increases to zero on the west rim, and +40 on the east rim. The higher value on the east seems to be associated with basic igneous rocks in this area.

QUANTITATIVE CONSIDERATIONS

A basin filled with sediments lighter than the surrounding rocks represents a deficiency in mass as compared with surrounding areas. This deficiency will cause a deficiency in gravity as compared with surrounding areas. Except for a factor which depends on the shape of the basin, the amplitude of the negative anomaly will be proportional to the thickness of the basin sediments, and to the difference in density between the sediments and the surrounding material.

The densities of the surface rocks may be as low as 2.0, if they consist of unconsolidated Tertiary material. But a number of investigators have demonstrated that the densities of unconsolidated and poorly consolidated sediments, especially clays, increase with depth of burial, due to compaction under the weight of the overlying material. Thus it is improbable that any basin can contain more than a few thousand feet of very light material, and in a basin as deep as 30,000 feet the average density of the material will probably not be less than 2.5. The density of basement rocks probably averages about 2.7, and the average density difference is therefore probably not greater than 0.2. Neglecting the shape factor, a 30,000 foot basin with this density difference would yield a gravity minimum of only about 75 milligals. In order to account for 200 milligals, as is found in the Po valley, we would have to make the basins very much deeper, probably at least 100,000 feet, for, extrapolating observed compaction data, the deeper portions of such a basin would approach the basement density of 2.7, and have very little effect on the value of gravity.

Most geologists would reject the idea that as much as 100,000 feet of sediments have ever accumulated in one spot. This suggests that the gravity anomalies are not to be explained entirely as representing the density deficiency of the sediments which fill the basin. The gravity map of India shows a minimum associated with the Cuddapah basin. The sediments in this basin are of Pre-Cambrian age, and according to the Indian Geological Survey, they have an average density greater than that of the surrounding metamorphic rocks. Glennie explains this minimum as a downwarp of the sial layer, 10 km. in thickness, into the denser basaltic layer; and this layer, in turn, he pictures as being warped down into the still denser dunite layer. On this picture the sediments form only the upper layer of a downwarp that involves a section of the basement at least 30 km. thick. Density stratification does not stop at the base of the sediments, but extends into the upper part of the crust itself, as is suggested by seismic data.

Applying this hypothesis to the Po basin, for example, it seems possible that a half or two-thirds of the anomaly there could be accounted for by the downwarp of the basement, leaving only 75 or 100 milligals to be accounted for by the sedimentary fill.

A somewhat similar hypothesis has been invoked by Meinesz and Hess to explain the large gravity minima found at sea, associated with island arcs.

ISOSTATIC CONSIDERATIONS

According to the theory of isostasy, Bouguer anomalies should bear an inverse relation to topography; high areas should have negative Bouguer anomalies, low areas positive anomalies. While this rule seems to hold in general, the deep sedimentary basins constitute a remarkable class of exceptions. Most of the basins are lowlands, and surrounded by higher lands. Moreover, the gravity minima are not substantially reduced by making isostatic corrections; in fact, in most cases the isostatic correction has the opposite effect, and increases the difference between the stations in the basin and on the adjacent older rocks.

Moreover, as isostatic anomalies are ordinarily computed, no consideration is given to the fact that a basin containing a thick section of light sediments, even though at sea level, must contain a compensating mass of abnormally dense material at lower levels to be in isostatic balance with the surrounding areas. If we assume that such compensating masses exist, they must have a positive effect upon the pendulum; thus we should subtract this effect in calculating the isostatic anomaly then. If this correction were applied to the isostatic anomaly profiles, the amplitudes of the basin minima would be a third or a fourth larger than they are on the basis of the usual calculation. And since even these are difficult to account for except by assuming deep crustal downwarp, which is itself a negation of isostasy, we must conclude that the basins are not compensated at depth, and are actually out of isostatic balance.

THE MECHANICS OF BASIN DEVELOPMENT

The accumulation of a thick series of shallow water marine sediments in a trough or basin demands, of course, that the floor of the basin of deposition sink as the sediments are deposited. There is a widely held belief that the weight of the sediments is the cause of the sinking. But this theory demands that at every stage in the process the basin area is either in equilibrium, or has an excess of mass as compared with surrounding areas. The gravity anomalies, however, indicate that these basin areas have a deficiency of mass at present, and if the theory of crustal downwarp is valid, there must have been some deficiency throughout the period of deposition. This means that the sinking cannot have taken place because of isostasy, but rather in spite of isostasy.

The gravity anomalies indicate that the floor of the basin has been forced down into a position which is lower than its position of equilibrium, and in opposition to the isostatic tendency. This must have required a force of considerable magnitude.

In this connection it may be significant that the largest negative anomalies are found in basins which are adjacent to folded and overthrust mountain ranges. This relation suggests that the orogenic forces that produced the great horizontal movements in the mountain belts also produced the downwarps in the adjacent basins.

If the downwarps were formed by orogenic forces, acting in opposition to isostasy, then the continued existence of the downwarps, in positions far below their isostatic levels, must mean either that the same forces continue to operate, holding the basins down, or that the earth's crust is sufficiently strong to support fairly large isostatic anomalies, once they are formed. The width of the Po anomaly is about 200 kilometers; and some of the anomalies reported by Meinesz in the East and West Indies are more than 400 kilometers in width.

The gravity work of the past twenty years has revealed a great many exceptions to the theory of isostasy, which were unknown at the time that the theory was formulated. Isostasy is not nearly as perfect as was formerly supposed, and near-surface densities will not account for all of the observed discrepancies. There seems to be need for a general revision of our ideas regarding isostasy and the strength of the earth's crust, in the light of the new data.

SECTION OF BIOLOGY

FEBRUARY 8, 1943

PROFESSOR ROBERT CHAMBERS, New York University, New York, N. Y.: *Tissue Culture Method in Recent Investigations.* (Illustrated by motion pictures.)

One of the handicaps under which tissue culture has long labored is the tendency of the investigator to make the growing of cultures an end in itself. However, the value of the efforts in cultivating all possible kinds of tissues and in identifying the varied types of cells migrating out of the implanted fragments cannot be minimized. The cytological aspects of tissue culture are of paramount importance and a knowledge of the cells is essential for any purpose to which the method is being applied. The main criticism levelled at the method has been due to numerous published articles on environmental effects by investigators who have paid little or no attention to the particular cells involved and have drawn irrelevant conclusions on growth effects from the over-all spread of the cultures. However, once the fact is recognized of cellular specificity, the preëminence of tissue culture in recent investigations is in its service as a tool for problems of more general physiological interest. The mere fact that a culture for experimentation consists of living tissue is insufficient. An apparent exception to this is the use of the technique in the cultivation and multiplication of viruses where the mere presence of living cells, such as of embryonic tissue or of adult brain, in the suspension seems to be all that is required.

Another and more critical application of the method is the study of various physiological reactions of different types of cells and of organized cellular tissues. In the field of cell behavior there is the problem of the chemotactic reaction of leucocytes. Considerable work, both *in vitro* and *in vivo,* has been done in the past with results which are too much at variance to be conclusive. One of the difficulties in working out the problem is well illustrated in tissue culture. The procedure of preparing a culture consists of placing a fragment of tissue (the explant) in a drop of liquid plasma which serves as the medium. The plasma soon clots with the formation of minute strands of fibrin radiating from the centrally located explant. The cells, migrating out of the explant, move along the strands which serve as substrate for attachment. They move along lines of least resistance and give the impression of a directional movement which need not be ascribed to chemical influences. Misinterpretations regarding chemotactic movements in general have been due to the lack of recognizing the existence of such fibrin strands in biological media.

Our method of studying chemotaxis has been to fill glass microtubes with solutions or suspensions of the substances to be tested and, after sealing one end, to place these tubes horizontally, with their open ends in the vicinity of a mass of leucocytes in a plasma clot. The preparation is then mounted in an incubation chamber set at the appropriate temperature. If the substance in the microtube is positively chemotactic, the leucocytes, moving about in the clot, leave the clot and accumulate within the tube. By means of this method no attraction was obtained with hyper- or hypo-tonic salt solutions, amino acids, proteins, peptones, acids, or alkalies either organic or inorganic. Positive effects were obtained with all carbohydrates used; namely, solutions of sugars and suspensions of starch, glycogen and even cellulose. Positive effects were also obtained with inflammatory exudates and sterile filtrates of broth in which staphylococci had been incubated for 10 hours or longer but not with a suspension of staphylococci in saline solutions alone. The attracting agent in the inflammatory exudate is thermolabile and loses its potency on standing.

Another field of investigation has been the secretory function of the proximal tubules of the kidney. In the controversy, several years ago, over the question whether certain dyes are secreted or are absorbed by the proximal tubules the matter was settled by a study of the problem with mouse metanephric and chick mesonephric tubules in tissue culture. The aspect of tissue culture made use of in this investigation was to observe the fragmented segments of the tubules in the explants in which the cut ends healed over by a proliferation of the epithelial cells exposed by the cut. These closed segments exhibited striking evidence of secretory activity by a progressive distension of their lumina with tubular urine. More recently it has been possible to show that the destruetive action of x-radiation is considerably greater if the tubule is incited to active secretion than when it is not. Also, it was found that the damage done to the cells may be reversible. For example, when the tubules are submitted to a given X radiation and, immediately thereafter, caused to secrete actively, the chance of damage is much greater than when the tubules are allowed to remain in a comparatively resting state for 10–12 hours before being caused to secrete.

Significant results concerning problems of cell mechanics have also been obtained with the kidney tubules in the cultures. Phenolsulfonephthalein (phenol red) is actively secreted and the secreting cells of the tubules acquire the yellow color of the compound indicating an intracytoplasmic pH of slightly less than neutrality. On the other hand, cells which are undergoing mitosis in the wall give no evidence of taking up the compound although no such discrimination occurs with vital dyes such as neutral red. The difference between the acquisition of color by the cells with neutral red, a basic dye, and with phenol red, a highly dissociated acid dye, is to be explained by regarding one as due to physical diffusion and the other to active secretion. The staining with neutral red is by physical diffusion and occurs with all living cells irrespective of variations in their activity. On the other hand, the uptake of phenol red depends upon a specific behavior of given cells. A cell undergoing mitosis is not in a state of secretory activity and therefore does not color with phenol red.

Another interesting feature of the kidney cultures concerns the epithelial cells which do not become incorporated in the newly forming wall at the cut end of the tubular segments. These cells migrate away from the implanted fragment and form epithelial sheets. The cells in the spread-out sheet do not color with phenol red. However, after a period of time a group of cells in the sheet begin to show color. It is then found that these cells are clustered around incipient cavities which progressively enlarge. These cells have recovered their secretory ability, are passing phenol red through their bodies and accumulating it with fluid in an ever enlarging cavity which they surround. This brings up the interesting suggestion that orientation plays an important role in the acquisition and maintenance of specific functions by the cells.

Another important field is the use of tissue culture in a study of the specific action of water-soluble vitamins and hormones. A direct and relatively rapid method for this study is to free the culture-medium of all its water-soluble constitutents and to observe the effect of immersing the washed cultures in a physiological salt solution containing the substances to be tested. The cultures of a given tissue are first grown for 12–24 hours in the usual medium of a plasma clot. During this period, cells from the implanted fragment grow out, the epithelial cells forming expanding sheets and the wandering cells forming clusters which radiate about the explant. The washing of the cultures with a salt solution does not disturb the cellular outgrowths which are held in place by the fibrin of the clot. When a physiological, non-nutrient medium is added the cells remain alive for some time but exhibit no further growth. One of the substances we have tested was ascorbic acid (Vitamin C). Vitamin C, added to a non-nutrient medium of Tyrode (a physiological solution containing NaCl, KCl, $CaCl_2$, glucose and $NaHCO_3$), induced an active proliferation of outgrowing cells. The presence of the vitamin does not seem to be necessary to maintain the intercellular cement which binds the epithelial cells together. Its action, rather, is of a tonic nature, enhancing intracellular metabolism. The extracellular cement has been found to depend entirely upon the presence of calcium in the medium.

Tissue culture has also a very important place in the study of cancer. Tumor tissues generally form luxuriant growths when cultured *in vitro.* Attempts have been made to compare them with cultures of their normal prototypes. Recent studies have been aimed at a comparison of the effects of various organic compounds on the cultures of normal and cancerous tissues. Some of these compounds have been found to be highly specific in their action. For example, dimethylparaphenylenediamine will destroy the glandular cells of the liver without affecting the duct-cell epithelium or the fibroblastic tissues. This seems to occur with the formation of certain tumors in the liver of rats fed for months with dimethylaminoazobenzene. This compound accumulates in the liver where it is, in all likelihood, broken down into simpler compounds of which one is dimethylparaphenylenediamine. The resulting cholangiomatous tumor is evidently an overgrowth of the ductal epithelium.

Another interesting case is the action of tetramethylorthophenylenediamine on lympho-sarcomatous and leukemic nodes in contrast to its effect on normal lymphoid tissue. This compound, at sufficiently high concentrations, is toxic to all tissues. However, the concentration can be reduced until it is non-toxic to fibroblasts, wandering cells and normal lymphocytes but still toxic to the malignant lymphoid elements of the lymphomas. There is a considerable margin of safety since a concentration which destroys the lymphoid elements of the malignant lymphoid tumors is about seven times weaker than the concentration sufficient to destroy the other, presumably, non-malignant cellular elements of the tumor and the lymphocytes of normal lymph nodes. Unfortunately this compound is toxic when given *in vitro.*

The investigations described above are mostly of the short-term type in which a given culture is seldom maintained for much longer than a week or ten days.

Long-term experiments are those in which the cultures are carried on to test the effect of long-continued sojourn in given environments. They have demonstrated the constancy of cell types and have disposed of the earlier suppositions of cellular dedifferentiation. An apparent exception to this is the conversion of polymorphonuclear leucocytes into cells resembling monocytes by the gradual retraction of the multiple lobes of the nucleus until the nucleus becomes simple ovoid. However, these cells maintain their specific property of exhibiting positive chemotaxis, a property which true monocytes do not possess.

An extremely significant phase of long-term experiments is the attempt to use carcinogenic agents in producing malignant tissues *in vitro*. The test for this is transplanting the cultures into an animal since it is known that malignant tissues, grown for varying lengths of time in tissue culture, readily produce tumors when thus transplanted.

In conclusion, a survey of recent investigations demonstrates that the tissue culture is one of the most valuable adjuncts in experimental cell physiology.

SECTION OF PSYCHOLOGY

FEBRUARY 15, 1943

DOCTOR THEODORA M. ABEL, Letchworth Village, Thiells, N. Y.: *Negro-white interpersonal relationships in a limited environment.*

The patterns of behavior of negro and whites toward each other vary, as we know, with the cultural milieu in which they find themselves. The castelike system, of course, pervades more or less all the cultural media in the United States but its rigidity breaks down in certain spheres of activity, in different geographic areas or regions as the North and South, or in urban or rural communities. This rigidity varies also in groups of different socio-economic classes or occupations, in periods of economic prosperity, depression, crises or war, and under various legislative and political procedures.

For the social scientist an ideal condition is afforded for studying variations in negro-white interpersonal relationships, when both the negroes and whites are removed from their usual cultural environment and placed in an environment that is overtly different from the one to which they are accustomed. This happens when negroes and whites are placed in custodial and penal institutions that have little or no segregation along color lines. In these institutions the barriers of our castelike system are let down by constituted authority more than they are in most outside communities, for the negroes and whites eat, sleep, work, bathe, go to the toilet and have their recreation together.

What happens then to interpersonal relationships under these conditions? In order to make clear some of the changes in interpersonal relationships that do take place, I propose to describe the behavior of negroes and whites in one limited environment, that of a large New York State institution for mental defectives, and to explain some of the reasons for this behavior.

The institution in which this investigation was made lies in a rural district 40 miles from New York City. The patients are committed to the institution by the courts because of their mental deficiency and inability to make a social adjustment in the community. The group from which we obtained most of our material consists of some 680 adolescent girls and women of whom 60, or 9 per cent, were negroes. The girls in this group are housed in eight cottages in each of which are two wards of 40 beds each. Each cottage is supervised by a matron and three or more attendants. These employees are all white women. Discipline is maintained by authoritarian methods. There is no discrimination as to which cottage negroes are to live in or where they are to sleep except that care is taken not to have more than a dozen negroes in any one cottage at a time, for it is the opinion among staff members that a group of negroes larger than this becomes unmanageable.

The average I.Q. of the 60 negro girls is somewhat higher than that of the white girls (58 for the negroes and 50 for the whites) but because of the great number of white girls there are many more white girls than negro girls with the highest I.Q.'s in the group (up to 80). On the average, the negro girls are younger than the white girls by some 8 years or so, but here again, because of their large numbers, there are many white girls as young as the youngest negroes (C.A. 16-17). The parental backgrounds of both negroes and whites are similar in that the majority of fathers fall into the class of semi-skilled labor.

The most outstanding modes of behavior of negroes toward whites can be divided into three major categories under the headings of aggression, leadership and sex. These different aspects of behavior often go hand in hand, but for purposes of clarification we shall discuss these three characteristics of behavior separately.

Negroes show marked aggressive behavior toward white girls. Among the 60 negro girls under observation, 35 can be considered markedly aggressive. They dominate the recreational periods by dancing in a noisy manner and shoving white girls out of the way, by determining what music is to be turned on, what games are to be played. They go out for sports (basketball, baseball and skating) and show aggression by participating most actively in games, by shouting and giving orders. In work situations also they boss and play dominant roles in their relation to white girls whenever opportunity offers. For instance, one negro girl bosses several white girls in the laundry, telling them to get busy and stop being lazy.

Negro girls always seem to be meddling in the affairs of other girls, protecting their friends, shoving and nudging girls they do not like out of the way. White girls do this too, but more particularly when negroes are not present. Whenever three or four negroes are present in any recreational group, no matter how large, provided these negroes are friends, they dominate the group by being noisy, by shouting, laughing, and bossing white girls.

Leadership as here difined takes place when an individual plays a dominant role in a specific group studied, a role in which she is able to attract several other members of the group and to exert an influence over their attitudes and behavior. Leadership is frowned upon by the institution authorities and attempts are continually made to prevent a girl from becoming a leader. This is done by transferring her from a cottage if she gets a foothold there, and, if this fails, by paroling or discharging her from the institution.

Only negro girls have become outstanding leaders. In the course of two years' observations, there were five outstanding negro leaders, and eight minor leaders. Some of the minor leaders had at one time been leaders or potential ones but were kept under strict supervision by institution authorities so that there was little possibility for their playing a dominant role. An outstanding leader can influence not only other negro girls but has a following of 25 or more white girls. The minor leaders influence at the most a half dozen to a dozen girls. Only two white girls approached leadership during this time and they could be considered only minor leaders.

One negro leader, Alice (I.Q. 67, C.A. 22), was a tall, light-colored negress, who had been institutionalized since the age of 11. She had never been a sex delinquent and had been institutionalized so long that her leadership was not based on experience in the community and "superior knowledge" of worldly affairs. Alice was liked by all the girls, negro and white, who came into contact with her. She had a following of at least 50 white girls and a dozen negroes, all of whom said, "Alice is my best friend." Among her followers were girls she had never even spoken to but who admired her greatly. Some did not "dare" to speak to her. Alice was quiet, she never raised her voice. She was very diffident toward employees and members of the staff, but she was nice to girls. She looked out for their interests, fought their battles verbally (for she was never assaultive). For a while, and before it was discovered, many of her followers waited on her hand and foot. These girls disobeyed rules and washed her underwear, for they said Alice should wear clean clothes each day. They got her coat for her when she went out, took off her shoes when she returned from work. They even washed her sanitary napkins. One girl said, "Alice shouldn't touch such things." Her followers gave her their trinkets, food or other gifts they received from home. Her lock box contained an assortment of pins, bracelets, hair clips and necklaces.

There came a time during the winter when Alice succeeded in having most of the cottage (93 girls) under her control, to say nothing of girls in other cottages who knew her either at work or because of previous cottage experiences with her. Attendants in Alice's cottage found that girls who formerly had caused no trouble were beginning to be disobedient. One girl who was asked to do something for an attendant replied: "I cannot do that without asking Alice first." On another occasion an attendant picked up a garment and asked a girl to put it away. The girl's reply was, "That belongs to Alice, we'll have to speak to her." On the basis of these cues indicating the influence she was exerting, Alice was kept home from the movies one night. When this news was learned in the cottage, two white girls came to the matron and begged to be allowed to take the punishment for Alice. They said she should not be punished and if two of them "took the punishment" for her, all would be well. When this request was not granted, Irene, one of the two girls, began to scream. This girl became so excited that she had to be placed in seclusion for 24 hours.

The third outstanding characteristic of the negro girls in the institution is the sex behavior they elicit on the part of white girls. The attraction negro girls have for white girls is a phenomenon reported as occurring among girls and young women in penal, corrective and custodial institutions. Among the subnormal girls we have observed, there are a great many white girls who are outspoken about calling a negro girl their best friend or "my only friend." The interpersonal relationships between negro and white girls range all the way from a more casual friendship to a very strong homosexual attachment. In the majority of cases, however, there seems to be no marked desire for physical contact beyond hair-pruning or walking arm in arm. The white girls derive a great deal of pleasure out of writing letters to a negro and receiving one in reply, or showering a negro girl with presents. Some white girls admire a negro leader (major or minor) from a distance and only go so far as to wait on the negro but rarely talk to her in any sort of equal relationship. These girls give the negro presents, usually through the intermediary of another white girl, and expect only a smile or a kind word in return. Other white girls assume a more overt sexual relationship, flirting first with one negress and then with a second, so as to arouse the jealousy of the first. On one occasion a white girl started a fight among four negroes at the breakfast table. She had flattered one after another only to turn each one down in turn. Just what preceded the climax was not ascertained but the four negroes began calling each other names and throwing paper cups at each other. Sometimes two white girls are rivals for the affections of a negro girl. In the course of our observations, a negro girl, Winifred, paid more particular attention to one white girl in a service building than to the other white girls who admired her. The relationship was not overtly homosexual but was enhanced by the flattery Winifred felt from the excessive fawning of the white girl. After some time another white girl, who worked in the employee's kitchen, took employee's food and served it to Winifred on a tray. Winifred was pleased with this attention and shifted her affection to this girl. The first girl, Sally, became incensed. She called the new admirer, Anna, all kinds of names, including that of "negro lover," a name that has a very highly colored emotional tone in the institution. The new admirer retaliated and Winifred came to her rescue by hitting Sally over the head with a rolling pin.

In considering the sexual behavior of the negro and white girls it must be remembered that in the institution the more usual community emotional relationships such as heterosexual ones and those centering around parental affection are entirely removed. These girls are almost completely segregated from boys and men. It is true they talk occasionally to their physicians, dentist and doctors in the hospital. They see boys mowing lawns and doing outdoor work on the grounds and can flirt at a distance or say "hello." But their isolation from the male is greater than in any situation in the community. Parental affection is also lacking. The matrons and attendants of a cottage have no time to play a maternal role with a group of 80 or more girls. They are often sympathetic and many girls become attached to them, but the girls cannot receive the individual attention a parent or a housemother in a small group can give. Not only are men and parents removed from these girls but so are their children. Quite a few of them have had children (among the 60 negroes, 13 had one or more children). In a control group of 60 whites of equivalent I.Q. and C.A., 7 had one or more children.

It can be said that generally the negro girls play predominantly the "male" role in their relationship to the white girls, at least the "male" role as it is determined by society today in which the male makes sexual advances, accepts the admiration of the female, becomes jealous when the female flirts with another male, sometimes orders the female about and expects her to do the male's bidding and sometimes shows off to attract the attention of the female. But often in their relationships with one another, negro girls who have been acting a "male" role toward a white girl will turn around and play a "feminine" role toward another negro girl. For instance, two negro girls who are loved by white girls are aggressive and domineering toward the white girls but toward other negro girls they often become passive and coy. They flirt and giggle, dance in the role of female partner who is led around the dance floor.

In partial explanation of the modes of behavior we have been discussing, we may say first that institutionalization enhances the aggressive tendencies of the negro girls, not because they are under restrictions but because they find themselves for the first time on an equal footing socially with white girls. They sleep, eat and bathe with them and no discrimination is made as to the type of work they are to do. They begin to feel that since the white girls have lost so much prestige as to be no longer in a "master caste" that they are in some way in an inferior position. Certainly, the negro girls do not look up to or admire the white girls. They find that the white girls admire them and that they can boss them and order them about. A white girl who will literally get on her hands and knees and remove the shoe of a negro and put on her slippers for her, or a white girl who brings a negro a tray of food "stolen" from the employee's kitchen, is no longer the negro's "mistress." Even if there are only a very few negroes who can command this marked subservient behavior on the part of white girls, the other negroes witness this behavior which cannot fail to have some effect on them.

It must also be remembered that institutionalization probably lowers the prestige of these lower class negro girls in their families and in their social group in the community less than it does the white girls who seem to be more acutely ashamed of their institution commitment. This relative importance of loss of freedom no doubt influences the negro and white girls differently.

Finally, we must not forget that we are dealing with white girls of subnormal intelligence who have been made well aware of this fact by their failures in school and in some of their difficulties of adjustment in the community. These white girls are used to being handled by individuals more intelligent than themselves. The negro girls are not necessarily more intelligent as far as I. Q. rating goes than are the white girls whom they boss and dominate, but their aggressive behavior must have the same effect on the white girls as that of being managed by individuals of higher intelligence; the white girls respond to the negroes as they would to individuals in authority. It is true the negro girls have also failed in school and had difficulties in the social world but they find in the institution a situation more favorable in many respects than they had previously experienced in the community, and one in which they can release their pent-up feelings of aggression on individuals who for the first time they can look down on rather than envy.

Since the negro girls are aggressive and dominant, it would be expected that the outstanding leaders should come from their group rather than from the white group. But, when we investigated the types of interpersonal relationships existing among negro and white boys, of chronological ages equivalent to those of the girls, although we found the negro boys aggressive and taking the initiative in athletics and recreational periods and showing bossiness toward white boys, there was no negro in the group whose influence could compare with that of either the major or minor leaders in the girls' group.

There are, of course, other factors besides aggressiveness and bossiness that make for successful leadership. There is the ability to dominate and manipulate in a manner acceptable to those who are willing to be led, and on the other hand there is the influence exerted by followers in maintaining a leader and in bringing yet more followers into the fold. These factors are present when leadership occurs in any group in society. But we feel that still another factor must be included in order to explain the very successful leadership of the negro girls. This factor is the marked sex attraction the negro girls have for white girls. In the male group, the negro boys do not have such a strong attraction for white boys. Occasionally a homosexual relationship is formed between a negro and white boy but never does the negro have a great number of admirers who overtly express their feelings toward him as do the white girls toward a negro leader. As further proof of the prominent part played by sex in the leader-follower relationship among the girls, we found that among the younger girls under 14 there is no negro leader; in fact, at these younger chronological ages, the white girls are often the leaders in a cottage and boss the negro girls. But when the white girls develop their secondary sex characteristics and puberty is well established (usually around 14 or 15), and they become definitely interested in sex objects, then they start admiring the negro girls and begin to play a subservient and "feminine" role toward them. The aggressiveness of the negro girls is interpreted in some way by the white girls as "maleness." Negro boys to be sexually attractive to white boys would need to assume a "feminine" role as dictated by our culture; they should be coy, flirtatious and submissive rather than aggressive. Since the male white group is not made up largely of passive homosexuals, the aggression of the negro boys has no particular sexual significance to them.

We have indicated the main reason why the negro girls become the male substitutes for the white girls; namely, that the white girls interpret the aggressiveness of the negro girls as a "masculine characteristic." But also, the uninhibited emotional expressions of the negro girls as revealed particularly in their dancing, games and rough-housing, provides the whites with the only available and accessible source from which they can obtain satisfaction to some degree for their emotional needs (sexual or parental). Most individuals follow along the easiest path to a goal. The goal here is emotional response (sexual or parental) from another individual, and since that response is more easily forthcoming from a negro girl than from another white girl, the white girl will turn to a negro girl for this response.

In conclusion we may say that the interpersonal relationships between negro and white girls, taking place in a limited custodial environment, do not allow us to generalize about the aggressive and leadership characteristics of all negroes, those in or out of custodial and penal institutions, those with normal and superior levels of intelligence, and those living in various regional areas. We should particularly need to investigate what happens to negro-white interpersonal relationships under conditions where negro girls would comprise the great majority of the group under investigation, the white girls forming a small minority. In fact, many more studies need to be made in order to provide us with a thorough understanding of the conditions under which negroes will dominate and be sexually attractive to whites.

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SECTION OF ANTHROPOLOGY

FEBRUARY 22, 1943

MISTER GEOFFREY GORER, Institute of Human Relations, Yale University, New Haven, Conn.: *Themes in Japanese Culture.** (This lecture was illustrated by lantern slides.)

Superficially, contemporary Japan would seem to be the most paradoxical culture of which we have any record. How can the same culture—often the same persons—prize and perform the elaborate, highly ritualized and symbolic Tea Ceremony, with its elegance, calmness and poetry, and indulge in the almost unbelievable savagery, lust and destruction of the rape of Nanking? Go out by the hundreds of thousands to admire the wild cherries in bloom or to listen to the cicadas, and at the same time systematically and consciously force whole populations into the degradation of drug addicition? Hold serious lyrical poetry competitions, in which the Emperor is a contestant, and build a shrine for the Living Bombs—three soldiers who fastened themselves to a high explosive bomb? Develop some of the most refined graphic arts we know, and yet have a major portion of the work of their most famed artists so pornographic that much of it has never been seen in Europe or America? Adopt the most elaborate complications of our modern society and yet retain a view of the world—part political, part religious, part social—more consonant with an isolated and primitive tribe than with a major industrial nation?

It was an attempt to explain these paradoxes that led me to undertake the research which I shall attempt to outline this evening. I have tried to isolate theoretically those aspects of the Japanese character structure and culture which might account for these and other paradoxes and which can be hoped to have some predictive value in dealing with the Japanese. To do this I have been forced to simplify the total picture enormously and to omit large portions of the culture; in particular I omit any but passing mention of the politico-social structure of power, and of yet another paradox—that Japanese society and economy have very little similarity with the other great Asiatic societies and culture areas (China, India, Java or Egypt) but far more resemblance to Europe, particularly Medieval and Renaissance Europe; nor can I elaborate on the strange socio-economic parallelism between the capitalist-imperialist policies and ideologies of feudal Japan and Nazi Germany. In a total picture, of course, these points would have to be given full consideration.

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* This paper is a condensation of my memorandum. *Japanese Character Structure and Propaganda*, prepared for the Committee on Intercultural Relations, and published by them in mimeograph, March, 1942.

Probably I should have started with an apology and an explanation. I have never been in Japan; I cannot read Japanese; I have no special qualifications for discussing Japanese culture. The research that I undertook at Yale was in the nature of a methodological experiment; an attempt to interpret culture by the application of fairly rigid theoretical formulations to such data as were available in books written in European languages, supplemented by interviews with some two score informants who were either wholly or partially Japanese, or who have had prolonged and intimate knowledge of Japanese life. Since such validity as can be granted to the conclusions depends on the acceptance of the methodology, you will perhaps allow me to outline my basic postulates in some detail.

These postulates are derived from the disciplines of social anthropology, psychoanalysis and stimulus-response psychology, and represent a first attempt to construct a unified theory of social science. These formulations are inevitably tentative. There are 12 of them.

(1) Human behavior is understandable: with sufficient evidence it is possible to explain any observed behavior, however incongruous isolated items may appear.

(2) Human behavior is predominantly learned. Although the human infant may be born with some instincts and is born with some basic drives whose satisfaction is necessary to its survival, it is the treatment which the infant undergoes from the other members of the society into which it is born, and its experiences of its environment, which are of preponderating importance in molding adult behavior. (In this context I may as well state that I have assumed consistently that the genetic peculiarities of the Japanese do not involve any inherent psychological differences from other groups of human beings.)

(3) In all societies the behavior of the component individuals of similar age, sex and status shows a relative uniformity in similar situations. This is equally true in unformulated and unverbalized situations.

(4) All societies have an ideal adult character (or characters, depending on sex and status) which is of major importance for the parents in selecting which items of their children's behavior to reward, and which to punish.

(5) Habits are established by differential reward and punishment, chiefly meted out by other members of the society.

(6) The habits established early in the life of the individual influence all subsequent learning, and therefore the experiences of early childhood are of predominant importance.

(7) The chief learning in early childhood consists of the modifications of the innate drives of hunger, optimum-temperature seeking, pain-avoidance, sex and excretion, and of the (probably learned) drives of fear and anger (anxiety and aggression) which are demanded by the adult members of the society; consequently a knowledge of the types of modifications imposed, the means by which they are imposed, and the times at which they are imposed, is of major importance in understanding adult behavior.

(8) Since everywhere it is predominantly the parents who reward and punish their children, the attitudes of the child to his father and mother, and, to a lesser degree, toward his siblings, will become the prototypes of his attitudes toward all subsequently met people.

(9) Except in situations of the greatest physiological stress, adult behavior is motivated by learned (derived, secondary) drives or wishes superimposed upon the primary biological drives.

(10) Many of these wishes are unverbalized or unconscious, since the rewards and punishments which established the habits of which these wishes are the motives were undergone in early childhood, or because the verbalization of these wishes was very heavily punished; as a derivative from this hypothesis, people frequently cannot verbalize their motives, which have to be deduced from the observation of what satisfactions are actually obtained in a given situation.

(11) When these wishes, acquired through early training, are shared by a majority of the population, some social institutions will eventually be evolved to gratify them; and existing social institutions, and those that are borrowed from other societies, will be modified to congruence with these wishes, insofar as this is possible without impeding the gratification of the primary biological drives.

(12) In a homogenous culture the patterns of superordination and subordination, of deference and arrogance, will show a certain consistency in all spheres from the family to the religious and political organizations; and consequently the patterns of behavior demanded in all these institutions will mutually reenforce each other.

On the basis of these assumptions I constructed a very detailed questionnaire on childhood training (the possible variants of the treatment of infants are limited) which I administered to my informants. I sought for, and obtained, answers in the most concrete possible forms. In other words, I collected from my informants not generalizations but reminiscences of concrete family situations—in the case of Nisei and Japanese autobiographical reminiscences. The generalizations are my own. I listened to those which my informants insisted on making, but did not necessarily accept them. When the majority of informants reported similar behavior from different social contexts I assumed this to be the pattern. When an informant reported practices not reported by others, an attempt was made to check this information; if no check could be obtained the information was not incorporated, on the theoretical ground that the individuals observed were probably aberrant. Simultaneously I read the majority of the available books in English, French and German, to build up a picture of the existing adult society and to search for confirmations or negations of the hypotheses deduced from the interviews.

Owing to the fact that I could interview only people in the northeastern United States, my sample produced a definite distortion. The vast majority of my information deals with people living in the cities, and of middle and higher income groups. I have no consistent information at all on the urban proletariat, and my information on rural society is almost entirely dependent on the book and conversation of Dr. John Embree, whose study of the village Suye Mura is incidentally the only book on contemporary Japan using modern sociological concepts and techniques.

I will now proceed to outline summarily the main patterns of Japanese childhood training, and then attempt to explore the development of some of these themes in adult social life.

Almost without exception the Japanese welcome children, especially male children. Much fuss is made of the new baby, especially if it is a boy, and it quickly becomes the focal point of the household. For the first month the baby is held in the arms of its mother; after that it is carried spread-eagled on the back, with a double sash holding it under the arms and under the bottom. In winter a special kimono covers mother and child; such fresh air as the child gets is obtained by bending its head sideways.

The oldest child is carried entirely by its mother; but subsequent children will also be carried by older siblings. This is the one domestic task which is imposed on boys up to the age of about ten, if there are no sisters to carry the new baby.

During the first two years of life the child is on the back practically all its waking hours. When on the back the child has to learn to adapt itself passively to the often violent movements of its carrier; a mother will do arduous household tasks, or a sibling play tag or hopscotch, while burdened with a baby.

The Japanese baby is, if anything, over-fed. It is not only given the breast whenever it shows any signs of wanting it, but the breast is also often offered to it before it shows any signs of hunger. Also from a very early age the child is given a little solid food, at first premasticated by the mother and spat into the baby's mouth; a little later finely ground rice and gruel are given directly. Frequently, when the baby is fed solid food, the mother's hand is held over its mouth to prevent regurgitation.

Normally the child is weaned shortly before the birth of the next baby, generally between the age of 2 and 3. Weaning is apparently gradual and accomplished entirely by verbal prohibitions and exhortations; superficially it would seem to be very slightly traumatic. Children are allowed artificial pacifiers if they desire them. They also are allowed to put any loose objects into their mouths; and thumb-sucking, though uncommon, is not interfered with.

According to Occidental standards, Japanese babies are over-protected against cold. They are dressed in several layers of clothing. In winter the warmth of the carrier's body is held in by the special kimono which covers mother and child. The child sleeps under warm quilts, and one of the chief causes of infantile accidents is being smothered with the quilts.

From birth, the Japanese baby is restricted in its movements. The constricting position in which it is carried has already been described. If the child fidgets or cries while on the back, the carrier joggles from leg to leg until the baby quiets down. This shaking is described as rhythmic, and therefore may have a soothing quality; but since it is sharp and sudden—a sort of jumping from foot to foot—it is almost certainly predominantly painful, and the child quickly learns that any expression of pain or anger or any undictated movement is dangerous. When the baby starts to crawl it is forcibly restrained. The Japanese house is dangerous for a baby, and is conceptualized as even more dangerous than it is; a baby can break the paper walls or burn itself on the open charcoal burner; and it is believed that even the weight of a baby may be too heavy for the raised joists which support the house. These joists follow the shape of the white mats; and as soon as a baby can walk he has to learn to avoid stepping on the joints between the mats. One of the greatest crimes that a child can commit is to step on the sill, for that, it is said, would risk having the whole house put out of shape; if a child of four or five does so, he may suffer the very severe punishment of having the cautery, *moxa*, burned on its back. The Japanese child has to "learn" its home completely, until, even when burdened, there is no risk of stepping where he should not; a mistake involves not only the possible incidental pain of falling or being burned, but also severe punishments from the parents.

Besides learning where to walk, the Japanese child from the second half of its first year onward is carefully taught how to sit. The correct Japanese sitting position is almost certainly painful for young children (some people consider it the chief cause of the very general Japanese bandy-leggedness), especially when it has to be maintained without any fidgeting or unnecessary movements of the hands or head; but the absolute standard is aimed at and nearly attained in very early life, partly by reward, partly by positively-phrased exhortations, and partly by shaking, pinching and other punishments. This strongly sanctioned early training in keeping quiet in uncomfortable positions is reflected in adult life in the often noted impassivity (inscrutability) of the Japanese; they have been strongly trained not to give physical expression to their emotions. Incidentally, this impassivity and physical restraint is the first item of behavior to be reversed under alcohol.

Further bodily training is given to Japanese babies from the age of about six months, in bowing to people to whom courtesy is due; the mother or carrier puts her hand behind the baby's neck or upper back and pushes the top part of the baby's body forward, with the appropriate expressions. By the time a child can toddle it should be able to make these salutations spontaneously, or at a word from its guardians.

There is a considerable aura of fear and anxiety about the etiquette of sitting and bowing. Parents are anxious to avoid the criticism which will be made to them if the child does not behave appropriately; and the child is frightened that it will be punished by its parents if they are criticized.

In the life of Japanese children, the most consistent and most severe aspect is cleanliness training, training in control of the sphincter. For about the first four months the child has a heavy cloth diaper (not paper, which is used for the disposal of all adult excreta) which is changed relatively rarely, though the child is regularly washed in hot water. After four months the child is held out over the balcony or road at frequent intervals, either when it cries, or when its guardian considers the time is ripe; any lapse from cleanliness is punished by severe scolding, the mother's voice expressing horror and disgust, and often also by shaking or other physical punishment. The training is meant to be complete by the time the child can toddle on to the balcony and all informants agree that this ideal is obtained.

Except those Japanese who have accepted Western standards of prudery, or in those situations where Western disapproval might be manifested, Japanese parents are extremely permissive toward infantile expressions of sexuality. Even when these are directed toward adults, they are usually treated good-naturedly, as funny; the only negative sanction used is mockery, but, for the Japanese, this has very considerable emotional importance.

Physical modesty, as an absolute, is not inculcated by the Japanese (except that they should not be seen defecating). Immodesty is defined as a greater exposure of the surface of the body than the situation calls for. Verbal modesty is treated similarly; children know obscene words and there is no taboo on their using them, except in unsuitable circumstances. No attempt is made to hide from children the "facts of life."

Somewhere between the ages of 3 and 5—depending on when the next baby is born—the Japanese child finds that he has to adjust to a new picture of the world. It is expected that the Japanese child will share the attitudes of the rest of the family to the new-born younger sibling, will accept it as the center of the family picture, willingly give way to it, always be ready to play with it and listen for its first intelligible sounds. The child is expected to take a quasi-parental attitude toward the new baby, looking on it as a toy, rather than as a rival.

Whether a younger sibling hastens the event or not, somewhere around the age of 4 the Japanese child has to pass from babyhood to childhood. In infancy and babyhood boys and girls are treated alike or nearly alike (for it is possible that girls never get quite as much affection and attention); they are the center of attention, constantly played with and gratified and talked to, provided they follow scrupulously the rules of cleanliness and politeness, and respond passively to the adults around them. As they enter childhood, however, differential demands are made on boys and girls, and different behavior insisted on. The girls must still remain passive, but they are no longer the center of attention, and are no longer so much rewarded for their passivity; instead they are severely punished for any lapse therefrom, for any gesture of self-assertion that they may make. As they grow up, the restrictions on their movements, gesture, posture and language are constantly increased, reaching their culmination at marriage. There seems to be a conscious fear among Japanese parents that their daughters will grow up inadequately feminine; the greatest praise that can be given to a girl or her parents is that she is *onnarashii* (like a girl) or *otonashii* (quiet and inconspicuous); the term *otokorashii* (like a boy) is the worst insult that a girl can receive, and the most humiliating that a parent can hear. It is worth noting that the converse is not true of boys; even the most effeminate and cowardly boy is so superior to any woman, that there is no equivalent term for "sissy."

Realistically, there is good reason to fear that a girl will resent the social role laid out for her, and will try to adopt the role of her brother. Although no psychological change is demanded of her, she is fairly abruptly shifted from being the center of family attention to being the most insignificant member of the household. She learns that she has to give way immediately and unquestioningly to any demand or request made by any male (in babyhood even a girl baby is deferred to); that she has no claim for preferential treatment in any sphere; and that she also has to give way to all older females. She has many duties, few rights or privileges. Although her physical movements are controlled, she is allowed relatively free expression of her emotions; in contrast to her brother, the Japanese girl is volatile, easily changing from sorrow to gaiety, from tears to laughter, without reproof.

The world of the growing girl is not particularly indulgent, but it is self-consistent, and demands from the girl a (modified) continuation of her previously rewarded habits of passivity. The readjustment of the boy is much more complex.

Put briefly, the boy must learn to adapt differentially to two contrasting worlds. Toward his father, his elder brothers, and later his teachers he must preserve the same attitudes of passive compliance. There are absolutely no situations in which a boy must not show complete obedience and compliance to males superior to him in age or status; any lapse therefrom is immediately and drastically punished. But, because he is a male, he finds himself in a position of dominance over all women, practically without respect to age or status. Because he is a boy he has automatic precedence over all the females in his household; they must and do give way to him; demanding and aggressive behavior, which is so heavily punished when directed toward other males, is heavily and constantly rewarded when directed toward females.

This dominance of a boy of four or five over his female relatives is carried to very great lengths. In a dispute with a sister both parents will insist on the girl giving way; in a dispute with the mother, the father will back up the son. It is only the boy's physical strength which limits the aggression which he may display toward his mother, nurse or sister. The child learns that aggression, directed toward the proper persons, always pays. If the boy wants something that the mother refuses (or if he has something which the mother attempts to take away) he will go into a temper tantrum, lying on the floor, kicking and screaming. The mother will first try to calm him by calling him pet names and giving words of praise; if this fails she will offer candy; next she will try to cajole him with promises of future indulgences. But none of these devices may be successful; the boy may refuse to be diverted; and then he may strike and insult his mother, may kick her and punch her and bite her. The mother begs for mercy, begs him nót to hurt her; she may not use her greater physical strength to resist her son (at any rate in the urban middle or upper classes, where the economic value of a wife is so much less than in the country or among workers) for then her husband may punish her. As a final humiliation, the boy may destroy his mother's hair-do, and break her precious hair ornaments; but usually she will have given in to the little fellow before such a pitch is reached.

This excessive permissiveness toward boyish aggression is, as far as I know, unparalleled in other societies; but all informants agree that it occurs in this exaggerated form, and give detailed and circumstantial reports.

Although his mother is the one person against whom the Japanese boy is permitted to show almost unlimited aggression, and to whom he is required to show practically no formal respect, she nevertheless still remains the source of all his major gratifications; and the strong emotional bond between the mother and her son is normally never completely destroyed.

These violent temper tantrums stop when the boy is six or seven. At that age the boy will be going to school, and there, or anywhere else outside the home, all physical expressions of violent feeling are punished, laughed at and stopped.

The one other point which perhaps needs documentation is the early development and excessive importance given to the symbolic punishment of mockery. In all societies, being laughed at is felt to be unpleasant; in Japan, however, it is the most drastic sanction of all, more dreaded by most than the greatest physical pain and deprivation. In earlier periods, when a samurai gave his note to return a borrowed sum, the only guarantee affixed was the permission to be laughed at in public in case of failure. The mainspring of action in the *Forty Seven Ronin*, the most popular of all Japanese epics to this day, is the fact that the master of the Ronin was forced to commit ceremonial suicide because he had been wrongly instructed in court etiquette and was therefore laughed at during an audience with the emperor. This fear of mockery can be endlessly documented from the literature and from the observations of every traveller to Japan from the most superficial to the most studious. The very unpleasant feeling of fearing mockery, of bashfulness, is called *hazukashii;* and the fear of being mocked, and thereby "losing face" is a major motivator of Japanese behavior.

I am not altogether settled in my own mind as to the mechanisms by which hostile laughter takes on so great a significance. It would appear that when a baby makes any lapse from the proper behavior for its age—lapses from cleanliness, from politeness, mistakes in speaking—the onlookers laugh, half-embarrassed, half amused, and that subsequently the parents punish the child. In this way, hostile laughter would become a cue for subsequent punishment and withdrawal of love by the parents; and, although the punishments are subsequently forgotten in their concrete form, the fear of some vague and excessively terrifying punishment remains, for which the hostile laughter of others becomes the premonitory symptom. Theoretically this seems the most probable explanation; unfortunately it is not nearly so well documented by my informants as the other aspects of Japanese childhood training.

With this I think the roster of the major themes of Japanese childhood training is complete; and I will now try to follow through the ramifications of some of these themes in adult life. I shall restrict myself to three themes: the learning associated with the control of the gastro-intestinal tract, the learning associated with the attitude to parents, and the learning associated with the control of one's own body (including the expression of the emotions) and the social and inanimate environment.

The hunger of Japanese babies, as was pointed out, is, if anything, over-satisfied. "Hunger anxiety"—an unrealistic fear that one may at some time in the future be completely without food—does not appear to be a component of the Japanese character, nor is it given ritual elaboration. The rites connected with agriculture, and the assuring of a good harvest, though of some importance, especially in the country, do not appear to have any strong emotional aura. Appetite, as opposed to hunger, would appear to be lowered by the occasional stuffing which children receive, and devices for tickling the palate have been very little developed in Japan. This is one of the most marked superficial contrasts with China; and although there are numerous Chinese restaurants for the wealthy and sophisticated in the cities, the arts of Chinese cooking have not been imported into the Japanese home. The emotional unimportance of food is also exemplified by the well-documented indifference of the Japanese to regularity of meals or of diet; far more easily than most Occidental peoples the Japanese, under the stress of competing drives, seem willing to postpone the satisfaction of hunger.

For adult Japanese the functions of food are somewhat disguised. For the leisure classes the aesthetic appeal to the eye, or the poetic associations of the name of the dish, are far more stressed than the appeal to the palate; and the ordinary people have given sexual connotations to the name of nearly every article of common diet. One might almost say that there was an aura of shame surrounding eating. It is noteworthy that in a culture which has ritualized almost every aspect of daily and ceremonial life, there is almost literally no Japanese table etiquette; one eats at a separate table, and while eating can give vent to all those unseemly bodily noises which are otherwise unacceptable. There is no conversation at meals; one gets eating over as quickly as possible.

Besides the obvious implications of this attitude toward food, I should like to suggest that it is a derivative of the Japanese attitude toward cleanliness training; that the unpleasant effects surrounding defecation have moved forward to the eating of solid food, and that both aspects of the digestive process are treated almost identically.

A further derivative of this training is the excessive Japanese fear and dislike of dirt. Japanese cleanliness is so marked that every superficial account dwells on it—cleanliness both of the body and of one's possessions. Because of the psychological implications, it is worth noting that the excessive cleanliness of the Japanese is not absolutely complete. It is rigorously applied to the body, the visible surfaces of the Japanese house and garden, and to all public buildings built in Japanese style. Public buildings of Western style, on the other hand, and such "neutral" places as railway coaches and waiting rooms, are likely to be far dirtier than their Occidental equivalents. Enemy country and captured towns are at best "neutral"; and consequently a Japanese conquering army is likely to be even dirtier and more destructive than their European counterpart.

These neutral areas, however, are relatively unimportant and cannot counteract the excessive fear of soiling and dirt. This fear would seem to be the fundamental psychological motive at the basis of religious Shinto ritual and belief. I quote from Sir George Sansom's *Short Cultural History of Japan:*

"The outstanding feature of Shinto observances is the attention paid to ritual purity. Things which are offensive to the gods were called . . . *tsumi* . . . Chief among the offenses to be avoided was uncleanness. It might arise in many ways, none of which necessarily involved what, in other religions, would be moral guilt. Uncleanness of the person, from mere dirt, was scrupulously avoided, and it was a necessary preparation for religious observances to wash the body and put on fresh garments. Sexual intercourse, menstruation and childbirth were regarded as causing ceremonial impurity, which must be removed by lustration, abstention and prayer . . . Diseases, wounds and death were also sources of uncleanness . . . Sickness and all the external signs of disease, such as sores, eruptions and discharges . . . were also defilements . . .

" . . . The list of offenses does not reveal any distinction between ceremonial impurity and moral guilt. The consummation of a marriage is no less defiling than adulterous intercourse, a blow or a wound pollutes both parties to a conflict, and generally we find that the early religion is almost entirely deficient in abstract ideas of morality. Its code is not ethical, but customary and ceremonial. It reprobates as sins only such acts as are visibly or immediately repulsive . . . .

". . . What is abhorrent, what must be washed away and expiated is not guilt but pollution. The concept of sin, as distinct from uncleanness, is wanting or rudimentary, and throughout their history the Japanese seem to have maintained in some measure this incapacity to discern, or this reluctance to grapple with, the problem of Evil."

Outlined in this quotation are the ethics peculiar to the Japanese—"The concept of sin, as distinct from uncleanness, is wanting." This too I would derive from the drastic toilet training. In all human societies, the innate drives of newborn infants have to be controlled in ways acceptable to the adults of the society, and this control is mostly taught very early, and chiefly by punishment. Two of the chief drives which must be controlled are sex and excretion. Where infantile sexuality is prohibited, it is prohibited absolutely; there are no places and no occasions when a child is allowed to strive for whatever sexual pleasure is physiologically possible for him. Where this prohibition is enforced rigorously, there would appear always to be moral absolutes in the value system of the society: absolute prohibitions, absolute sins, a concept of absolute evil, a constant contrast between the unreachable ideal (of purity in the first place) and actual mundane practice. Excretion, on the other hand, cannot, for physiological reasons, be absolutely forbidden; all that can be forbidden is excretion at the wrong time or in the wrong place. If this cleanliness training lies at the bottom of the value system of the society, it would follow that there would be no absolutes, no "right" or "wrong," but instead very strong emphasis on doing the right thing at the right time, on the minute following of ritual, on physical and ceremonial cleanliness, on "correct" or "suitable" behavior, which would be defined by the context in which the behavior took place. This would seem to fit the case of Japan.

Since there is so much emphasis on behavior suitable to a given situation, the drive to adapt to any new situation is very strong, since the sanctions for non-adaptation are among the most terrifying the average Japanese experiences. Consequently, although the Japanese are bound by a conservative ritualism in the Japanese environment, they are likely to be as adaptive as they can in any foreign situation. Also the sanctions for correct behavior and the standards of correct behavior which are operative in a Japanese environment would be no longer operative in a different environment and under different circumstances; and consequently all the aggression and cruelty which is unsuitable in Japanese contexts can be allowed vent.

In the last few remarks there is implicit yet a further derivative from the early toilet training; that is the emphasis on neatness and tidiness, on there being "a place for everything and everything in its place" (which might be considered the motto of toilet training). The Japanese carry this emphasis extremely far; their interior decoration and landscape gardening are constantly admired examples of this minute preoccupation with correctly placed detail; not a leaf or a piece of paper may be out of place. This perfectionism reaches perhaps its highest point in the highly ritualized Tea Ceremony, where every detail is clean, controlled, and free from intrusion. Similar manifestations are the minutely regulated rituals, most highly elaborated around the Court, but present at every social level and in every sphere of activity; if the ritual is completely and carefully copied, then there is no fear of "doing the wrong thing in the wrong place."

In contemporary psychology an individual preoccupied with ritual, with tidiness and order, is technically known as a "compulsive neurotic"; these characters with us are statistically unusual, and it is consequently justifiable to speak of them as neurotics. The Japanese character traits which I have just described, however, are statistically common, and therefore it would be unjustifiable to speak of the Japanese character as neurotic, unless one were appealing to some unformulated and universal ideal of the "normal"; the Japanese do, however, show most of the traits which in individuals would be called compulsive. In European compulsive neurotics, excessively severe early cleanliness training plays an important, indeed preponderating, role in the development of their symptoms.

Behind the rituals of the individual obsessive can always be discovered a deeply hidden, unconscious and extremely strong desire to be aggressive; the rituals are a psychological protection against the putting into effect of this dangerous urge. The reasons for this are neither spooky nor abstruse. Such early thorough training, before the baby has learned to speak, or can possibly understand the reasons for it, indeed before the appropriate muscles are really strong enough, can be produced only through severe and continuous punishment and by painful effort on the part of the child. The child automatically resents these pains imposed upon him and would like to destroy the author of them. But the child is too weak to gratify these desires; and later it learns that the source of these pains is also the source of all its pleasures and gratifications; the desire to destroy the source of all its pleasures, even though it inflicts pain, presents such a menacing picture that even the desire must be hidden, unnamed. Unfortunately it can never be destroyed.

Japanese society normally gives almost less opportunity than any other of which we have record, for the socially approved release of vicarious aggression; and, on theoretical grounds, one would therefore expect that this aggression is ready to be released in excessive quantities under suitable circumstances. This theoretical construct gives the best explanation for the striking contrast between the all-pervasive gentleness of Japanese life in Japan, which has charmed nearly every visitor, and the overwhelming brutality and sadism of the Japanese at war, which has horrified almost every observer and journalist and shocked the conscience of the democratic world.

With this I think we have carried the theme of the learning associated with the gastro-intestinal tract far enough (many may think too far); I should now like to pursue for a little the theme connected with the learning associated with the attitude toward the parents. This theme applies almost exclusively to male Japanese.

It will be remembered that the Japanese boy has to be continuously subservient and deferent toward his father, and elder brothers, but may be aggressive and commanding toward his mother and sisters.

Because of this differential treatment received from and allowed toward his two parents, the Japanese boy grows up in a divided universe where he must continuously use careful discrimination. To the male part of the universe—that is all males of superior age and status—he must respond obediently, passively and unquestioningly, following the patterns that they lay down and taking orders they give; only by so doing will he be safe and rewarded; any other action invites drastic punishment. Complementarily, as he grows older, he will learn that younger men and men of lower status will give him the unquestioning obedience that he gives others. The male universe is an ordered pattern in which reward is gained by compliance and a full knowledge of relative status. From the Emperor (with his divine ancestors as his superiors) to the newborn infant (with the outcastes as his inferiors) every male has his exact place in this male world; upward aggression will be punished directly by those affected, excessive downward aggression by mockery from those above. The male universe gives assurance and safety, but little indulgence; it administers the heavy punishments for deviance, controls the sanctions and all the secondary rewards. One must comply with the patterns of the male universe. Resistance and aggression are dangerous.

In contrast with this male world of dominance and submission, there is the female world from which come all the primary gratifications (food, drink, warmth, sex) and which can be controlled and forced to yield up its gratifications, by aggression or threats of aggression. The male world gives orders and expects obedience; the female world threatens and pleads, but can always be made to yield if the male is sufficiently strong and persistent in his aggressions. The female world is loved, ill-treated, and despised.

This division of the world of the male Japanese into two contrasting aspects is fully developed in Japanese symbolism and philosophy. Female is "dark" and "passive," male "light" and "active"; the land is male, the sea female, the sun male, the moon female, and so on. But this dichotomy has still further implications. The growing child sees the universe in terms of its parents; and this original division is never fully dissipated by later experience. It seems certain that to the contemporary Japanese other races and societies are viewed as either male or female: as groups to be followed and obeyed implicitly, or as groups to be forced to yield to aggression or threats of aggression. Further, it appears historically as though in the nineteenth century

England and America were viewed as indubitably male, and therefore to be obeyed and copied as much as possible; but that, in the last forty years, the sex of these groups has been first questioned, and then reversed. The defeat of the Russians in 1905 probably originally called in question the "virility" of the whites; and this uncertainty was increased by the willingness of the Anglo-Saxon to negotiate, to bicker and yield, instead of commanding, in the next twenty years. Finally, in the invasion of Manchuria, and subsequently, the Japanese found that the Anglo-Saxons showed all the female characteristics; they cajoled and threatened without carrying out their threats; they constantly yielded to a show of aggression, each time vowing that that concession would be the last; they parleyed; they allowed their nationals to be ill-treated or insulted without retaliation; they were feminine and would therefore yield the primary gratifications ("raw materials") if enough aggression were used. On December 7, 1941, this theory was put to the test; and the democracies still held their feminine role. Most convincing of all, they asked for mercy, declaring Manila an open city; and just as on such a plea the angry boy will destroy his mother's hair-do and break her precious pins, so in response did the Japanese destroy Manila with special attention to buildings of religious or symbolic importance.

Our final theme was the learning associated with the control of one's own body and the social and inanimate environment. It will be remembered that the chief social learning the young Japanese underwent was learning correct social behavior, how to sit properly and how to bow, and learning what parts of the house were safe and what were not. Furthermore, there was an aura of fear and anxiety about learning to sit and bow properly, for criticism or mockery by strangers would bring punishment from the parents.

From the moment a Japanese child can understand language—one might even without much exaggeration say from birth—standards of etiquette, of correct and suitable behavior under varying circumstances are demanded from him. As soon as the child leaves home these standards of correct behavior and adequate accomplishment are demanded to "honor the family name," so that "the family name shall not be disgraced." Recognition of this motive is given somewhat ludicrous expression in some of the arrangements of Japanese schools; every child is moved up in grade every year, and all the athletic events are so arranged that every pupil gets a prize. If one failed to do so, not only would the child itself feel unhappy, its whole family would feel disgraced.

This point of the individual representing a larger group is stressed from early childhood; the size of the group represented tends to increase as the individual grows in status; it is not uncommon for Japanese in foreign parts to feel and say that they represent the whole nation. The fear of criticism from strangers is one of the chief motivating forces for young Japanese during the whole of their childhood and adolescence; it is the most potent threat for enforcing conformity. While they are able to avoid such adverse criticism, the Japanese are given complete support and approval by their own group, especially the extended family; but if the individual arouses criticism in strangers, or if he fails to measure up to the demands made on a person of his age and sex (e.g. failing to get a rise in grade in a "modern style" school) then his own group will turn on him, punish him severely, even repudiate him if the criticism is strong enough. It is this situation which is probably at the back of the fact that most Japanese soldiers prefer to die rather than to be taken prisoner; a prisoner is considered weak and womanly, a person in whom no man should have any interest; to be taken prisoner means complete repudiation, means no group and no position to go back to after peace is declared. It is such a dismal alternative which makes death seem preferable, not a non-existent tradition of suicide. In peace time the suicide rate of Japan was less than a third that of parts of Germany.

It is perhaps worth emphasizing that the degree to which this repudiation by one's own group is carried is very rare sociologically. In most societies where the extended family or other fractional social group is operative, the group will usually rally to protect one of its members who is under criticism or attack from members of other groups. Provided that the approval of one's own group is maintained, one can face the rest of the world with the assurance of full support in case of need or attack. In Japan, however, it appears the reverse is the case; one is only sure of support from one's own group as long as approval is given by other groups; if outsiders disapprove or criticize, one's own group will turn against one and act as the punishing agents, until or unless the individual can force the other group to withdraw its criticism. By this mechanism the approval of the "outside world" takes on an importance probably unparalleled in any other society. The implications of this theme for the understanding of Japanese foreign policy appear so obvious that it is unnecessary to expatiate on them.

This approval from strangers is one of the ways in which the Japanese try to mold the environment to their satisfaction; but approval of free groups is always uncertain; people may withdraw their approval. Owing to the methods by which, and the society in which, the Japanese are brought up, no Japanese can feel safe and secure unless the whole environment is understood and as far as possible controlled. The uncontrolled and unknown are dangerous and frightening; from the small child who risks severe punishment if he does not know where the house-joists are, to the nobleman who risks shame and embarrassment if he does not know the exact rank, age and family of a new acquaintance, the unknown and uncontrolled are always equated with the danger of mockery from others and punishment by one's own group.

This desire for a controlled universe is manifested in a very great variety of ways: in the decoration of the houses and gardens, in which every detail is controlled and in its proper place; in the complaints so frequently made by foreigners that the Japanese open a friendly interview by inquiring the name, age, business, family position, etc. of the person they are addressing; in the meticulous observation, the note-taking, the photography, which are such persistent features of the Japanese abroad; in the minute copying, and thereby controlling, of foreign articles; and (as a converse of this attitude) in the extreme unwillingness of the Japanese to admit that they do not properly understand and control a foreign language, which has been the cause of endless imbroglios.

Besides these domestic features, Japanese history—especially the history of Japanese legislation—shows this constant urge to control the environment as completely as possible. Two consistent and only apparently contradictory trends can be seen running through the whole of Japanese history: these are the attempt to legislate against all change, so that the whole social environment is controlled; and secondly the unformulated belief that some non-Japanese society has a less oppressive solution for social regulation, and an attempt to impose that scheme or solution on the whole of Japanese society through minute legislation. Of the former trend, the most striking example was the policy of exclusion adopted for two centuries preceding the Meiji restoration; of the latter, the whole-hearted adoption of the forms of T'ang civilization in the eighth, and Sung in the twelfth century; and the adoption of

Western technology and ideas since 1867. This adoption has been extremely thorough and legislated; it is even reported that debates were held in the Diet as to whether April Fool's Day should be included in the Japanese ceremonial calendar. The consistent theme in both trends is the attempt to control by imposed authority whatever is strange and unknown in the environment.

During the period of exclusion, everything outside Japan was specifically recognized as dangerous, because unknown; so much so that a Japanese leaving the country was not allowed to return for fear that he should bring unknown and dangerous ways back with him. It may be an echo of this period—certainly an echo of this trend—that the most usual Japanese greeting—*O-kawari-gozaimazen-ka*—literally means "I trust there has been no change (since we last met)." Change implies unpleasant change.

With the reversal of policy since 1867 the danger of the uncontrolled outside world has not diminished; but now the Japanese are attempting to cope with the danger by dominance, instead of by withdrawal. It is a recognized principle of psychology that an organism faced with a frightening object will respond with either withdrawal or attack; after two centuries of the former method the Japanese have now tried eighty years of the latter. Their almost continuous history of aggressive warfare (Korea, China, Russia, the Triple Alliance, China, the United Nations) can, it would seem, be profitably viewed as recurrent attempts to control the dangers of an unknown and ever-expanding environment. Each portion of the environment subjected and controlled, however, leads on to ever more unknown and dangerous neighbors. This largely unconscious fear of the environment is insatiable. The Japanese can never feel safe unless, as some of their more bombastic military speakers have proposed, the Mikado rules the whole earth.

I have tried to develop three themes of Japanese culture from their impact on the young child to their remote effect on all of us in this country, indeed in the whole world. The selection of these three themes has inevitably been arbitrary, and it would have been possible to develop a number of others. The themes are those which seemed to me to be of most obvious relevance; and they have, I hope, served as a demonstration of a relatively novel methodology and approach to complex and relevant problems.

SECTION OF PHYSICS AND CHEMISTRY

JANUARY 8 AND 9, 1943

Conference on "*High Polymers.*"

The Section of Physics and Chemistry held a Conference on "High Polymers," the first of the series this year. Doctor Raymond M. Fuoss, General Electric Company, Schenectady, N. Y., was in charge of this meeting, as Conference Chairman.

The program consisted of the following papers:

"General Theory of Polyreactions and Some Recent Experimental Results," by H. Mark, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

"Elasticity and Flow in High Polymers," by Robert Simha, Howard University, Washington, D. C.

"Rigidities of Solutions of Polymers," by John D. Ferry, Woods Hole Oceanographic Institution, Woods Hole, Mass.

"Intermolecular Forces and Chain Configuration in Linear Polymers. The Effect of N-Methylation on the X-Ray Structure and Properties of Linear Polyamids," by W. O. Baker and C. S. Fuller, Bell Telephone Laboratories, New York, N. Y.

"The Structure and Mechanism of Formation of Addition Polymers," by Charles C. Price, University of Illinois, Urbana, Ill.

"Rate Theory and Some Physical and Chemical Properties of High Polymers," by H. Hulbert, R. A. Harman, A. Tobolsky and H. Eyring, Princeton University, Princeton, N. J.

"Statistical Theory of Chain Configuration and Physical Properties of High Polymers. Effects of Hindered Rotation," by Paul J. Flory and John Rehner, Jr., Esso Laboratories, Standard Oil Company of New Jersey, Linden, N. J.

"Thermodynamic Properties of Solutions of High Polymers. The Empirical Constant in the Activity Equation," by Maurice L. Huggins, Eastman Kodak Company, Rochester, N. Y.

SECTION OF PSYCHOLOGY

February 5 and 6, 1943

Conference on *"Psychosomatic Disturbances in Relation to Personnel Selection."*

The Section of Psychology held a Conference on "Psychosomatic Disturbances in Relation to Personnel Selection," the first to be held by this Section. Mr. Lawrence K. Frank, Vice President and Chairman of the Section of Psychology, was in charge of this meeting, as Conference Chairman.

The program consisted of the following papers:

"Introductory Statement," by Lawrence K. Frank.

"The Chief Structures and Functions Involved in 'Psychosomatic Disorders,' " by Donal Sheehan.

"Disturbances of Bodily Function in Relation to Personality":

a. "Hypertension," by Carl Binger.

b. "Gastrointestinal Disorders," by Harold G. Wolff.

"The Rorschach Method in the Study of Personality," by M. R. Harrower-Erickson.

"Disturbances of Personality":

a. "Detection by the Psychologist," by Gardner Murphy.

b. "Detection by the Psychiatrist," by Lawrence S. Kubie.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1943

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1943, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

Conditions:

(1) Eligibility. Authors and coauthors shall be members in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1943, to the Executive Secretary of The New York Academy of Sciences at The American Museum of Natural History, Central Park West at 79th Street, New York, N. Y.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1943 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,
Central Park West at 79th Street, New York, N. Y.

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Recording Secretary

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ELECTED FEBRUARY 25, 1943

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 5, APRIL, 1943 No. 6

SECTION OF GEOLOGY AND MINERALOGY[1]

MARCH 1, 1943

PROFESSOR MARLAND P. BILLINGS, Department of Geology and Geography, Harvard University, Cambridge, Mass.: *Ring-dikes and Their Origin.* (This lecture was illustrated by lantern slides and maps.)

INTRODUCTION

An *annular intrusive body* is one that is circular, oval or arcuate in plan. This is a rather general term that includes ring-dikes, cone sheets and other bodies. A *ring-dike* is a discordant, intrusive body that is circular, oval or arcuate in plan and has steep contacts. The annular shape is essentially that which the body possessed originally. A *ring-dike complex* is a structural unit containing one or more *ring-dikes*. *Cauldron subsidence* is a process involving the sinking of a cylindrical or paraboloidal block between steep walls. The resulting structure is also called a cauldron subsidence. Cauldron subsidence may result in the formation of one or more ring-dikes but the process does not necessarily lead to the formation of ring-dikes. Moreover, ring-dikes may form without cauldron subsidence, as will be shown below. Daly (1933, p. 269) uses the term *ring-fracture stoping* in preference to cauldron subsidence. It is apparent that the terms ring-dike, annular intrusive body, and cauldron subsidence are not synonymous.

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[1] No meeting was held in March by the Section of Biology. See page 155 for announcement of erence on "*Parasitic Diseases and American Participation in the War.*"

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Executive Secretary: Eunice Thomas Miner.
Assistant Editor Ethel J. Timonier.
Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

Although the term ring-dike was first used by E. B. Bailey (1914), the fundamental ideas were evolved in a study at Glen Coe, Scotland (Clough, Maufe and Bailey, 1909). The studies at Mull (Bailey, *et al.*, 1924) also contributed to the concept of ring-dikes.

SIGNIFICANT DATA

INTRODUCTION.—The writer has assembled data for 115 ring-dikes from 30 ring-dike complexes in different parts of the world. In making such a study from geological publications, an investigator is faced with many difficulties. Although in some instances the published descriptions are complete and fully adequate, in others the data are insufficient for a satisfactory analysis.

DISTRIBUTION.—Ring-dikes are extensively developed only in the British Isles and New Hampshire. They are undoubtedly present in the Oslo district of Norway, and may be common there. One ring-dike complex has been reported from Colorado and one ring-dike each has been reported from Utah, California, and Texas. Two ring-dikes reported from India and North Carolina have not been included in the present analysis, because the publications were not available to the writer.

NUMBER OF RING-DIKE COMPLEXES.—Approximately 30 ring-dike complexes have been described or are implied by geological maps. Eight of these complexes occur in the British Isles (four are Tertiary and four are Devonian); 15 occur in New Hampshire and adjacent areas; three occur in the Oslo district, Norway; one ring-dike complex each occurs in Utah, California, Texas and Colorado.

The data for the Oslo district are based on a study by the writer of geological maps. Holtedahl (1935) has shown that ring-dikes are present in the Oslo region but he gave very little detailed information.

NUMBER OF RING-DIKES.—Approximately 115 ring-dikes are known. Again it is difficult to be precise because in some instances it is rather arbitrary whether a structural unit should be considered a single ring-dike, made up of several separate intrusions, or whether it should be considered as several discrete ring-dikes. Fifty-nine ring-dikes have been described from the British Isles and 41 from New Hampshire and adjacent regions. At least 10 ring-dikes seem to be present in the Oslo region, probably more. Five ring-dikes have been described from the western United States.

The number of ring-dikes in a ring-dike complex differs greatly.

In many ring-dike complexes only one ring-dike occurs. On the other hand, in Ardnamurchan on the west coast of Scotland, 23 ring-dikes are found in a single ring-dike complex (Richey, Thomas, *et. al.*, 1930).

PLAN.—Very few ring-dikes form complete circles in plan. The average ring-dike forms a semicircle that encompasses about 180 degrees. Those that are complete encompass 360 degrees; an example of such is found in the Ossipee Mountains of New Hampshire (Kingsley, 1931). One ring-dike encompasses only 20 degrees of the possible 360 degrees.

No ring-dike is strictly circular in plan. In many instances there is a tendency to be elliptical, and all ring-dikes show irregularities to a greater or lesser extent.

RADIUS.—The radius of a ring-dike can be measured either to the outer or inner margin. On the whole, it seems best to measure the radius to the outer margin. The average radius of ring-dikes is 2.3 miles. The minimum value is about a thousand feet, whereas the maximum value, found in a ring-dike in the Franconia Quadrangle in New Hampshire, is 7.5 miles (Williams and Billings, 1938).

WIDTH.—The average width of ring-dikes, measured from the inner to the outer contacts, is 1600 feet; the maximum is 14,000 feet, in the Franconia Quadrangle, New Hampshire.

ATTITUDE OF CONTACTS.—It is highly important to know the attitude of the contacts of ring-dikes. Unfortunately, comparatively few data are available on this critical subject. It is clear that in many cases the field geologists have been unable to obtain this information despite assiduous search. In other cases, the published reports do not state clearly what data could be obtained. All together, about 43 observations on the attitude of the contacts have been recorded in publications. The average dip is 78 degrees outward. The lowest outward dip that has been observed is 33 degrees, found in the Belknap Mountains of New Hampshire (Modell, 1936). In many cases, the contacts are vertical and in a few instances inward. A dip of 78 degrees inward has been observed in the Percy Quadrangle, New Hampshire (Chapman, 1935).

DISCORDANT RELATIONS.—Many ring-dikes have been intruded into massive older plutonic rocks and thus it is impossible to determine whether the ring-dike is discordant or concordant. In some instances, however, the ring-dikes clearly cut across the structure of the older rocks. Some of the best examples of discordant relations are found in Norway.

SHIFTING OF CENTER.—Within a single ring-dike complex there may be more than one center about which the ring-dikes are concentric. Several ring-dikes may be concentric about one center. Other ring-dikes within the same complex may be concentric about a second center one or two miles away from the first. In general it seems that in Scotland the various centers within a ring-dike complex operated successively. That is, the earliest ring-dikes were intruded around one center. Then another center was established, and all ring-dikes intruded during this second stage were concentric about the second center. If a third center developed, the youngest ring-dikes were concentric about it. In New Hampshire, on the other hand, there is considerable evidence that two or more centers within the same ring-dike complex were operating simultaneously.

ASSOCIATED RING-FAULT.—It is believed that the intrusion of each ring-dike was preceded by the formation of a fracture. In some instances, it is possible to demonstrate that this fracture was a fault more or less circular in plan. The existence of a ring-fault has been demonstrated in at least three localities: Glen Coe (Clough, Maufe, and Bailey, 1909), Mull (Bailey, 1924, p. 340), and Belknap Mountains (Modell, 1936, pp. 1918–1922).

COMPOSITION.—Ring-dikes show a wide range in the rocks of which they are composed. In Scotland gabbro as well as granite is common in the ring-dikes. In New Hampshire the ring-dikes range in composition from monzodiorite through monzonite, syenite and quartz syenite to granite. In New Hampshire and Norway the rocks in the ring-dikes are alkalic.

CENTRAL VOLCANICS.—Eleven of the 30 ring-dike complexes that have been investigated for this paper have a central block of volcanics that has subsided. These central volcanics consist of flows and pyroclastic rocks; ranging in composition from basalt through andesite and trachyte to rhyolite, they are comagmatic with the rocks in the associated ring-dikes. These volcanics may occupy an area as much as 5 miles in diameter, but in most instances later intrusions have greatly reduced the area occupied by the volcanics. In some complexes, the volcanics are in direct contact with the ring-dike, but in others a belt of pre-volcanic rocks lies between the ring-dike and the volcanics themselves. The volcanics rest unconformably on these older rocks.

The volcanics dip inward toward the center of the complex. The dip is greatest near the ring-dike and becomes progressively less toward the center. Near the ring-dike the volcanics may even possess a vertical dip and in rare instances they are overturned so that they dip outward. Toward the center of the volcanic area the volcanic rocks may be essentially horizontal. The best example of a central subsided block of volcanics is found at Glen Coe, Scotland (Clough, Maufe, and Bailey, 1909). In the United States, good examples of central volcanics are found in the Ossipee Mountains of New Hampshire (Kingsley, 1931) and the Quitman Mountains of Trans-Pecos Texas (Huffington, 1942).

DISPLACEMENT.—Comparatively few data are available concerning the net-slip along the ring-fracture into which the ring-dike was intruded. In the Quitman Mountains of Texas the net-slip may not be much greater than 300 feet. In Mull the net-slip may be 3000 feet or more (Richey, 1932, p. 66). At Slieve Gullion, Ireland, there may have been no displacement along the ring-fracture (Richey, 1932, p. 99).

The central part of the volcanic block has always subsided more than that portion of the block near the ring-dike. In the Quitman Mountains the center of the volcanic block has subsided 4000 feet. In the Ossipee Mountains of New Hampshire, the subsidence of the central part of the volcanic block may be as great as 12,500 feet (Kingsley, 1931, p. 164).

CENTRAL STOCK.—Seventeen of the 30 ring-dike complexes possess what may best be called a central stock. Actually the center of the stock is almost always eccentric to the center or centers of the associated ring-dikes. The average radius of these central stocks is 2.0 miles. One near Sylling, Norway, has a radius of 3.8 miles; a very elongate one at Percy, New Hampshire, has a major radius of 5.5 miles, a minor radius of 1.4 miles. The central stocks usually consist of quartz-bearing rocks, commonly quartz syenite or granite.

SCREEN.—A screen is a body of country rock lying between two ring-dikes or a ring-dike and a stock. Screens are common in Mull but are rare in Ardnamurchan. They are fairly common in New Hampshire.

CONE SHEETS.—Cone sheets are dike-like bodies, arcuate in plan, that are concentric about the ring-dike centers. The thickness ranges from a few feet to a few tens of feet. Cone sheets dip about 45 degrees toward the center of the ring-dike complex, but the inner cone sheets dip more steeply than the outer ones. The Scottish cone sheets, if projected downward, meet at a focus about 3 miles beneath the present surface of the earth. It is commonly believed that cone sheets and ring-dikes usually occur together. This idea has developed because of their association in the British Tertiary districts. Actually only 3 of the 30 ring-dike complexes possess cone sheets; these 3 are Mull, Ardnamurchan and Slieve Gullion (Richey and Thomas, 1932). Moreover, many cone sheets occur without any associated ring-dikes.

VENT AGGLOMERATES.—Vent agglomerates fill passageways drilled through older rocks by explosive volcanic activity. In some instances the vent agglomerates form bodies that are arcuate in plan and thus seem to have been controlled by arcuate fractures. Vent agglomerates may form at almost any stage in the history of the ring-dike complex. Moreover, it is difficult in many instances to distinguish vent agglomerates from surface volcanic breccias (Bailey, *et al.*, 1924, pp. 195–210). Vent agglomerates are particularly common in Mull, Ardnamurchan and Arran; they have also been observed in the Quitman Mountains of Texas and the Belknap Mountains of New Hampshire.

AGE.—Ring-dikes older than Devonian have not been described, but there is no reason why pre-Devonian ring-dikes should not be found. Devonian ring-dikes have been described from Scotland. The ring-dikes of New England and adjacent parts of Quebec are probably Mississippian. The ring-dikes of the Oslo region, Norway, are Permian. A Jurassic ring-dike has been described from California, and Tertiary ring-dikes occur in Scotland, Texas, Colorado, and Utah.

ORIGIN OF RING-DIKES

ORIGINAL INTERPRETATION.—It is generally agreed that the intrusion of a ring-dike is preceded by the formation of one or more steeply dipping annular fractures. There are, however, two additional fundamental problems. One concerns the forces involved in the development of the ring-fractures. The second concerns the question of the origin of the space occupied by the ring-dike. The most complete discussion of these problems is contained in papers by Anderson (1936) and Richey (1932). In fact, although geologists working in New Hampshire reached some conclusions that they considered to be at variance with the ideas of the Scottish geologists, subsequent reading showed that our conclusions are almost identical with those of Richey.

The original interpretation was evolved by E. M. Anderson, whose theory was first expounded in the Mull Memoir (Bailey, *et al.*, 1924, pp. 11–12) and elaborated in a subsequent paper (Anderson, 1936).

A magma reservoir with a diameter of several miles is assumed to exist in the outer shell of the earth. The top of this reservoir is perhaps 4 to 5 miles beneath the surface of the earth at the time the ring-dikes form. The specific gravity of the magma is assumed to be equal to that of the surrounding rocks. Whenever there is an increase of pressure in this reservoir and the magma pushes outward, the surrounding rocks, because expanding, are subjected to tension. Tension fractures form perpendicular to the contact of the magma reservoir. If a cross section is visualized, the contact of the magma reservoir forms a parabola convex upward. The tension fractures, at right angles to this parabola, dip inward toward a common center, some distance below the top of the reservoir. Anderson points out that, even if actual fractures do not form, the surfaces perpendicular to the contact of the reservoir will be potential directions of weakness to be followed by any intrusions. These intrusions constitute the cone sheets.

On the other hand, if the pressure in the magma reservoir decreases, the surrounding rocks are subjected to tension. But the tension fractures under such conditions will be parallel to the contact of the reservoir. The outward inclination of such fractures is too low, according to Anderson, to be the ruptures occupied by ring-dikes. The ring-dikes, he believes, follow one of the two shear directions. Fractures following the steeper of the two possible shear directions would form a great arch. The block bounded by such a fracture, or fracture system, will have the shape of a paraboloid, convex upward. A potential cavity is formed between the country rock and the subsiding central block isolated by such a fracture. Magma welling up from the underlying reservoir fills this potential cavity as rapidly as it develops. After the magma has consolidated, the resulting igneous body will have the form of a cone opening downward, but the apex will be broad and round rather than sharp. Subsequent erosion of such a body produces an outcrop that is more or less circular in plan—that is, a ring-dike.

The essence of this hypothesis is that a ring-dike results from intrusion of magma into a potential cavity left between a subsiding block and stationary walls. The sides of the block must dip outward. The Scottish field geologists developed many subsidiary hypotheses which need not be elaborated here (Richey, 1932). This subsidence of a subterranean block constitutes underground cauldron subsidence.

In most instances in which the ring-fracture is essentially vertical and maintains its steep dip right up to the surface of the earth, volcanic rocks derived from the underlying magma reservoir have subsided within the ring-dike. This results in what is known as surface cauldron subsidence. It will be recalled that such central volcanics are found in 11 of the 30 ring-dike complexes that have been studied. In other words, 35 per cent of the known ring-dike complexes represent surface cauldron subsidence. Anderson grants that he has not worked out the dynamics of this type (1936, p. 154).

EVIDENCE FROM NEW HAMPSHIRE.—It became apparent as field work progressed in New Hampshire during the early 1930's that the hypothesis outlined above was not entirely satisfactory. In New Hampshire the contacts of the ring-dikes are essentially vertical. It is obvious that no cavity forms when a cylindrical block bounded by vertical walls subsides. It is also difficult to understand, on Anderson's hypothesis, what stops a block once it is isolated and starts to subside.

Vertical ring-dikes can be explained if we assume that the intrusion has been controlled by an annular vertical fracture zone, the width of which was comparable to the present width of the ring-dike. This fracture zone would be a place of weakness, very susceptible to piecemeal stoping. In some instances the stoping might proceed in the manner originally outlined by Daly, whereby small blocks settle in the magma to be assimilated at depth. On the other hand, many blocks might be carried along by magma moving upward along the fracture zone. The abundance of xenoliths, usually only a few inches across and partially assimilated, in the Albany quartz syenite, which forms some of the most conspicuous ring-dikes of New Hampshire, gives added weight to this hypothesis. The piecemeal stoping hypothesis also explains the irregularity of the contacts of some ring-dikes.

The importance of piecemeal stoping in the formation of ring-dikes was realized by New Hampshire field geologists during the years 1931 to 1934. J. E. Richey, who has had more experience with the ring-dikes of Scotland than anyone else, had already realized the possible importance of piecemeal stoping (1932, pp. 99 and 132).

There is another very interesting fact concerning the ring-dikes of New Hampshire. Although there are 15 ring-dike complexes and 41 ring-dikes, subsided central volcanics are found in only 5 of the complexes. Moreover, in every instance the ring-dike surrounding these subsided volcanics is composed of the same rock—the Albany quartz syenite. This indicates that surface cauldron subsidence occurred only at that stage in the evolution of the White Mountain magma series when the magma had evolved to a quartz syenite stage. Conversely, the

35 ring-dikes not composed of Albany quartz syenite were associated either with underground cauldron subsidence or with no subsidence. This subject is considered below.

ADDITIONAL MECHANISMS.—The Scottish geologists have suggested that in certain instances an arcuate block might be bounded by two essentially concentric ring-fractures that join; these fractures might be cut off upward by a horizontal fracture. An arcuate potential cavity would form if such a block were to subside. Intrusion of magma into the potential cavity, followed by consolidation and erosion, would result in an intrusive body arcuate in plan.

One argument advanced by the Scottish geologists in favor of this mechanism is the occurrence of "cappings" above some of the ring-dikes, notably the Beinn a' Ghràig ring-dike at Mull (Bailey, *et al.*, 1924, pp. 341–344), and dike A at Ardnamurchan (Richey, Thomas, *et al.*, 1930, pp. 285 286). There is also some evidence bearing on this subject in New Hampshire. In the mechanism developed by Anderson, a continuous ring-fracture occupying 360 degrees should be represented either by a ring-dike or a fault. At the south end of the Cape Horn ring-dike in the Percy area of New Hampshire, exposures of the older country rock are unusually good. It is apparent that there is no fault south of the ring-dike. Therefore, a mechanism involving the subsidence of a large, central, paraboloid block cannot be employed. The contacts of the Cape Horn ring-dike are relatively smooth, however, and it seems likely that the magma was emplaced by the subsidence of an arcuate block bounded by two arcuate fractures (Chapman, 1935, p. 419).

In some cases the magma may make room for itself by pushing apart the walls of a ring-fracture. This mechanism has been suggested to explain the concentric folds around the early granophyre at Mull (Bailey, *et al.*, 1924, pp. 168–169).

In some instances the intrusion of a ring-dike may be preceded by the introduction of vent agglomerates. In this case the older country rock is blown out. However, if the magma of the ring-dike occupies the space filled by the agglomerate we still must find some mechanism to make this exchange—either the magma pushes the vent agglomerate upward or to the side, or takes its place by piecemeal stoping.

The reasons why ring-dikes are usually arcuate, occupying only part of a circle rather than forming a complete circle, has been discussed by Billings (1942b, pp. 286–288) and Modell (1936, pp. 1925-1927).

Origin of Ring-fractures.—In the preceding paragraphs we have considered the mechanics of intrusion of ring-dikes. All hypotheses are similar in assuming that the intrusion is preceded by the formation of one or more ring-fractures. We have as yet not considered the forces involved in the development of these ring-fractures.

Anderson assumes that the ring-fracture forms when the magma in the reservoir is under reduced pressure. There are, however, several objections to this interpretation. Richey (1932, p. 134) has pointed out that it is difficult to conceive of magmatic pressure falling to such an extent that the roof rocks are subjected to tension. On the other hand, the mechanism recently described by Williams to explain such calderas as Crater Lake, Oregon, involves such a process. The magmatic pressure in the reservoir is reduced by the eruption of vast quantities of lava (Williams, 1942, pp. 101–107).

The contacts of most ring-dikes in New Hampshire are vertical. This is inconsistent with the outward dipping attitude deduced by Anderson in his analysis. Is it not much more likely that the ring-fracture is produced by magma pushing upward? Granite magma has a lower specific gravity than the solid older rocks into which the ring-dikes were intruded (Daly, 1933, p. 276). Under such conditions the granite magma would be constantly trying to rise, as Nettleton has shown in his analysis of the fluid mechanics of salt domes (Nettleton, 1934). Cone sheets form if the overlying roof-rocks fail by the formation of shear fractures (Billings, 1942b, p. 283). On the other hand, circular vertical fractures analogous to extension fractures may form (Billings, 1942, p. 103). Once a cylindrical block overlying the magma reservoir has been completely isolated from the surrounding rock by such a fracture it is then free to sink into the magma. Richey has also suggested that ring-fractures form when the magma is pushing on its roof (Richey, 1932, pp. 136–137).

IMPLICATION FOR STOCK INTRUSION

Clough, Maufe and Bailey (1909) discussed this problem in the Glen Coe paper. They pointed out that if a cylindrical fracture with a vertical axis were cut off upward by a subsurface horizontal fracture, the subsiding cylindrical block would not extend to the surface of the earth. The potential cavity left between the subsiding block and its roof would have the form of a cylinder. Intrusion of magma to this potential cavity would result in an intrusive body circular in plan and possessing steep contacts. To this type of intrusion the term stock is usually applied. The mechanism just described has been called underground cauldron subsidence. Although this idea was proposed nearly thirty-five years ago, the mechanism has not been employed by American geologists. The Scottish geologists have applied it specifically to the Glen Cannel granophyre at Mull (Bailey, *et al.*, 1924, pp. 331–336) and the Mourne Mountains in northern Ireland (Richey, 1928). Many stocks of the White Mountain magma series in New Hampshire have probably formed in this way.

This hypothesis has some advantages over the piecemeal stoping hypothesis as an explanation of stocks. Both underground cauldron subsidence and piecemeal stoping are adequate to explain the discordant character of the contacts of stocks. The relatively smooth contacts characteristic of stocks have been difficult to understand on the piecemeal stoping hypothesis. Moreover, as many geologists have realized for years, xenoliths should be relatively common if piecemeal stoping were an important mechanism. Underground cauldron subsidence explains the rarity of xenoliths.

OTHER INTRUSIVE MECHANISMS

In thus emphasizing ring-dikes and cauldron subsidence in the present paper, the writer by no means wishes to imply that this is the only way in which intrusive bodies may be emplaced. Unfortunately, many geologists, after studying a few igneous bodies and deducing the mechanics of intrusion, have concluded that their hypothesis applied to all large igneous bodies throughout the world. They have failed to realize that other mechanisms might be equally important.

New Hampshire is an unusually good place to study contrasting intrusive mechanisms. Whereas the White Mountain series has been emplaced largely by cauldron subsidence, the older New Hampshire series has been emplaced chiefly by forceful injection. Magma under the influence of orogenic pressure has forced its way into the older country rock. Moreover, atomic replacement has also been locally important in New Hampshire in the formation of what we commonly call plutonic rocks.

SUMMARY

Our ideas about ring-dikes have been developed almost exclusively by Scotch geologists, notably J. D. Richey and E. B. Bailey. In the present paper critical data concerning 115 ring-dikes in 30 different ring-dike complexes have been compiled. Ring-dikes are most extensively developed in the British Isles and in New Hampshire. The principles established by the Scotch geologists are applicable to New Hampshire. Although some modifications of the original theory were evolved independently in New Hampshire, subsequent reading of British publications showed that the same ideas were already in print.

The initial ring-fractures are essentially vertical extension ("tension") fractures due to the upward pressure of the magma. Some ring-dikes form when magma fills the potential cavity left between the walls and a subsiding block, the walls of which slope outward. Many ring-dikes, however, form when magma intrudes an annular fracture zone by piecemeal stoping. Although, in such a case, the block inside the ring-fracture may subside, this does not necessarily take place. Thus the formation of a ring-dike is not necessarily accompanied by cauldron subsidence. A few ring-dikes result from the subsidence of an arcuate block lying between two ring-fractures. In some instances ring-dikes have pushed apart their walls. Stocks may form when a potential cavity, left between the roof and a subsiding block, is filled by magma.

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SECTION OF PSYCHOLOGY

MARCH 15, 1943

DOCTOR EUGENE LERNER, Sarah Lawrence College, Bronxville, N. Y.: *Personality Development and the Social Psychology of Peace.*

In the struggle for democratic victory and lasting peace, the most serious bottleneck is not in the production of ships, tanks, airplanes or food, but in the production or training of more adequate personalities. This problem of psychological "production" exists both in the regions inhabited by the United Nations and, especially, in the countries dominated by Germany and Japan.

There is no longer time for leisurely debating about the question of so-called "cultural lags." It is pathetically obvious by this time that we are tremendously more efficient in coping with physical and chemical problems of transportation, agriculture, industry and physical health than we are in tackling biological, anthropological and psychological problems of human relations and personality development. The fact that we are up against such serious emergency problems in the present global crisis may actually be helpful in the long run. Some of the most important advances in science have been made through desperate attempts at coping with acute emergencies.

In the field of human relations, the basic and lastingly effective approach must be sought along the lines of prevention, instead of concentrating on essentially palliative treatment when the psychopathological condition is already upon us, so to speak. The problem is apparently one of preventive mental hygiene on a world-wide scale. Just as we managed to eliminate hampering superstitions and dogmas from sanitary engineering and medicine, so we must at once proceed to set up effective and enduring mental hygiene control of so-called political and economic problems. The combined biological, anthropological and psychological approach must try to accomplish here what physics and chemistry have done in the sheerly technological improvement of industry and transportation.

Though little is known from the standpoint of such scientific, largely psychological, control of human relations and personality development, that little is more than enough to start with—in comparison with the even more haphazard and obviously unsuccessful techniques of untrained persons. A little scientific knowledge of "human nature" applied now or very soon seems a safer gamble than just standing by, hoping to apply a whole lot of it later on. If the specialists and professional workers refuse to tackle the job at once, simply because all the discoveries needed are not yet available, even more ignorant and psychologically less realistic amateurs will continue to insist on pinch-hitting for them. We may then never accomplish the job "later," due to the dangerously unsound and irrelevant fumblings of so-called "practical" persons and untrained reformers, however well-meaning, who lack the perspective of contemporary biology, anthropology and psychology. Ours is truly, or must speedily be turned into an era of scientific, essentially psychological, planning.

We already have a good deal of planning in the field of public health and public education. The question is not whether we should have a planned society, but what kind of sounder planning should we have. What kind of planned human relations, planned personality development, planned education and re-education for social change may be envisaged? How can such psychological plans, themselves subject to change, be expected to be improved realistically and imaginatively as we go along and keep making psychologically relevant further discoveries? Let us try to answer these questions: (a) what kind of personality "production" seems indicated for a lasting and progressively more democratic peace, according to relevant clinical and research findings; (b) what could be done immediately, starting from where we are, in order to make possible such personality "production," including continuous improvements and not easily anticipated modifications?

Psychological methods are now available and could be further improved: (a) for sizing up the quality and trend of existing personality development and human relations; (b) for improving the prevailing quality and trends; (c) for checking up from time to time on the progress of psychologically relevant improvements in personality "production" here and abroad. Of course, there is need for further work and discoveries.[2] [3] Such work, geared to the needs of personality "production," should have priority over other kinds of experimental and clinical work for some time to come, even after the war. Just as industrial production can be co-ordinated and geared to the war effort, so the work and investigation of psychological specialists may be correlated more definitively from now on, with the help of such groups as the Society for Research in Child Development, the National Research Council, the

Society for Psychological Study of Social Issues, the National Committee for Mental Hygiene, the Social Science Research Council and others.

As an immediate starting point, we should aim for the global production of the kinds of personalities of all ages that are found now in Switzerland and Sweden, in the United States and England, in Canada and Australia—in contrast, say, to the kinds of personalities that are being produced now in Nazi Germany and similar countries. This means an inventory job of what we have on hand, to begin with.

What kinds of psychologically relevant differences could we expect to find to account for the personality characteristics and behavior trends of Swiss or Swedish boys in comparison with Nazi-educated German boys, of American or English girls in comparison with Nazi-indoctrinated German girls? It is unlikely that tests of visual acuity or memory, or tests of general intelligence and special aptitudes would reveal significant differences between the general populations living under such contrasting social orders.

We *could* expect significant group differences in such dimensions of personality as flexibility-rigidity, objectivity-subjectivity, directness-indirectness, emotional stability-volatility, dominance-submissiveness, and optimism-pessimism. Even at cursory inspection we could grossly expect that a Nazi-disciplined youngster or youth would very probably be much more rigid, much less objective, much more submissive toward persons of authority and much more aggressively domineering toward social "inferiors" or weaker persons than a Swiss or Swedish boy of comparable age, intelligence and educational background. He would probably also show characteristic varieties and fluctuations of behavior in other personality dimensions, in given behavior-sampling situations or interpersonal relationships.[2,5,6]

In addition to such "horizontal" differences at any given point of growth, certain developmental differences can also be gauged "vertically" as differential rates of social-emotional maturation. From infancy through adolescence, in given Western societies, the trend is broadly from excessive initial dependence to increasing self-reliance, from narcissistic and clannish self-centeredness to increasing objectivity, from emotional explosiveness and impulsive hostility release to greater self-control, more flexible frustration-tolerance and more constructive substitute-gratifications, from extreme infantile jealousy and competitiveness to relatively greater readiness for friendly team-work and affectionate mutual aid.[3] Research findings suggest that children reflecting more authoritarian adult influences display a slower increase of self-reliance in moral reasoning. In child groups subjected to autocratic leadership, impulsive aggressiveness markedly increases, while behavior indices of friendly mutual aid and team spirit show a decrease, along with diminishing constructiveness, objectivity and self-reliance. The opposite apparently happens in more democratically "led" or handled groups, just as clinical findings show greater all-round self-reliance in children exposed to relatively lesser degrees of adult domination.

In the case of adult personalities, such developmental trends may be similarly gauged. Differential rates of increase in presenile social-emotional involution and in regressions produced by severe frustrations can be expected. Such differential rates of adult personality development are reflected in sympathetic identification with, or active participation in, the Ku Klux Klan or lynching mobs, the Father Divine movement or the Townsend movement, etc. More rigid frustration-tolerance can result in extreme forms of conservatism *or* radicalism—in too rigidly resisting any social change whatever or in too rigidly insisting on particular, perfectionistically unrealistic, notions of social change. The point is that, while in the United States a Ku Klux Klan type of social movement remains relatively insignificant, in adult Nazi Germany it actually manages to become the dominant culture pattern.

In relating the differential rates of social-emotional maturation of children to those of adults, two further findings are important. On the one hand, childhood experiences partly determine adult personality organization. On the other hand, institutional patterns such as family relationships, school relationships or industrial relationships also partly determine the individual's state of mental health and social-emotional retardation.

We can then meaningfully ask: What environmentally manipulable and already manipulated influences produce the Swiss boy or the Swedish boy, the American or English adult, in comparison with what we get in the case of countries like Nazi Germany? How can we even further improve the former, how can we modify or re-educate the latter, with the Swiss or Swedish, the American or English examples as minimal standards for international, preventive mental hygiene?

The direction of more and more democratic personality "production" can now be gauged pretty well. It amounts essentially to this: autocratic or excessively authoritarian power-relationships must be curbed or eliminated throughout the life-span, and throughout the globe, in all basic interpersonal relationships between parents and children, teachers and pupils, employers and employees, public officials and citizens. In family life, in school life, in factories, courts, churches and hospitals, the individual's need and readiness for more secure freedom, privacy and self-reliance, for status and self-respect, for a feeling of belongingness and team spirit must be given ever more leeway. This is in keeping with the basic principles and methods of modern child guidance, progressive education, general mental hygiene and free inquiry in science.

Continued progress is needed in these matters even in the more democratic countries themselves. In addition, steps must be taken to spread these principles and methods of child education and adult education to the Nazi-indoctrinated and Nazi-dominated countries, regardless of the difficulties involved.

In postwar reconstruction programs, the Wilsonian principle of self-determination ought to be limited by such minimal requirements of international public health and mental hygiene. Complete and premature local self-determination is not sound to introduce or rely on everywhere, as long as it can result in a Nazi Germany and in the kinds of personality which are being turned out by its "education for death."[6] Transplanting and nursing along, for several decades or even longer, methods of representative government, progressive education and mental hygiene in family and industrial life, curbing economic and political nationalism in its manifestations on all age-levels, throughout the life-span—these are all minimal conditions for the accelerated production of more adequate and democratic personalities and cultures everywhere. If this can be considered turning Europe or other global regions "into a kindergarten," then we need more and better conducted nursery schools and kindergartens, both literally and figuratively speaking.[4] Any other approach is apparently doomed to failure, to judge from the basic fact of two world wars within one generation.

It is obvious now that it is very unsound and dangerous to plan the peace chiefly or even exclusively from an economic and military standpoint, instead of gauging the political, economic and military problems from these basic mental hygiene aspects of psychological reconstruction. This means that a great number of government personnel in the United Nations, members of congress, farmers, business and labor leaders ought to be made familiar at once with these perspectives afforded by modern biology, anthropology and psychology. Perhaps more physicians, psychiatrists, psychologists, social workers, educators, engineers and scientists in general ought to enter politics at once. To insure such minimal mental hygiene standards in public thinking, standards of personnel selection ought to be immediately raised, from a psychological and psychiatric viewpoint, with reference to both appointed and elected officials. It is doubtful, for instance, that the late Sen. Huey Long would have passed such criteria of personnel selection—any more than Hitler or Mussolini; and there are, unfortunately, still others, even within the United Nations. The common man and the psychological specialists cannot afford to take chances on "letting George do it" any longer, since George keeps doing it to all of us with terrifying ignorance and frequently psychopathic perseverance, even in the more democratic countries.

If the common man in the democracies were given a more adequate understanding of the terrible acuteness of these problems and tasks, the psychological specialists might be soon given a far greater opportunity for applying to so-called "purely" economic and political problems the plans and techniques based on the most relevant findings of biology, anthropology and psychology. The common man, whether government official, businessman, farmer or workingman, is not able directly to tackle the problem of building better houses or bridges, or of the prevention and treatment of typhus. But, once assured that expert knowledge is available, the common man could see to it that the proper specialists be called in on the job, instead of more or less well-meaning, untrained amateurs. While the common man is rather helpless in directly tackling highly specialized tasks in the field of human relations and personality development, the specialists cannot undertake and execute such projects without the active consent and support of the untrained majority. The common man needs the psychological specialist, and *vice versa.*

This means a quick yet sound job of translating and communicating with the common man—more intelligibly, more convincingly than ever before. It is a problem of much more effective communication and hastened adult personality development even within the democratic countries. The principles, results and methods of free inquiry embodied in the sciences of biology, anthropology and psychology must be quickly made to help the common man help himself, before it is too late.

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SECTION OF ANTHROPOLOGY

MARCH 22, 1943

PROFESSOR G. EVELYN HUTCHINSON, Osborn Zoological Laboratory, Yale University, New Haven, Conn. *Food, Time, and Culture.*

It is generally possible to study any system S in two ways, (a) the *hological*, in which matter and energy changes across its boundaries are studied; and (b) the *merological*, in which the behavior of individual systems of lower order composing S are examined. In the first instance, the behavior of S can be interpreted, if desired, in terms of an abstract mean member $\bar{s}$ of the class s_1, ($\bar{s}$, however, may have inconsistent properties). In the second case, the behavior of S is often predicted in terms of the aggregate Σs_1. Usually both methods are interwoven in any investigation, but the concept of a nation developed from the study of international politics, trade and finance would correspond to (a); that developed from a study of human behavior of individual members of the nation to (b).

The definition of S may be entirely arbitrary, or in terms of natural boundaries. The definition of a natural boundary with respect to any property is given as follows: When along any line drawn across the area under investigation any property p is found to vary with x, the length of the line measured from any arbitrary origin, the line is said to cross a natural boundary at any value of x for which p is discontinuous, or for which $\frac{dp}{dx}$ is maximal or minimal. Considering any set of properties $p_1, \frac{dp_1}{dx} \ldots \frac{dp_n}{dx}$ are not necessarily maximal or minimal for the same values of n; for different purposes the natural boundaries are different.

Biological communities have been studied in various ways. The extreme holological approach may be applied in examination of the *total mass of living matter per unit area*, and the closely related *ecological photosynthetic efficiency* of a community. Evidence exists that if, starting with a barren area, either on land or covered by inland water, there is a slow but accelerating increase in the total biomass, then an inflection in the growth rate and finally an approach to a maximum level (stage equilibrium of Lindeman) which in nature may be maintained for centuries and which presumably is determined geochemically (by H_2O, P, etc.).

Merologically the characters of a community may be considered in various ways, of which one is a study of the frequency of occurrence of species, or probabilities p_1, p_2, p_3, . . . that any organism taken from the community belongs to given members of a set of species a_1, a_2, a_3 . . . The number of species of probability in excess of some given value defines the *diversity* of the community.

It appears that the diversity increases with the biomass initially, then reaches a maximum, probably before the maximum in the biomass, and decreases later. There is evidence that throughout stage equilibrium the probabilities for individual species vary, though the diversity may not.

The whole community can be regarded holologically as characterized by at least the following quantities, varying with respect to time: the *biomass*, the *diversity*, and a *set of probabilities* relating to particular species. Variation in the biomass differs from that of the others in that antecedent states determine subsequent states.

A single non-human species shows comparable increase in biomass with time, but nothing that corresponds to diversity or probability sets, even in the most developed social insects.

If there is an analogy between a biological community and a human society, as has often been maintained, it should be possible to find properties of the latter, formally equivalent to the biomass, the diversity and the sets of probabilities.

If a series of biological communities, delimited by *natural boundaries*, and in equivalent stages of development and in the same geographical and climatic region be compared, the same specific combination of the biota need not be expected, because more species may have high probabilities of occurrence than there are spaces to fill. Similarly various aspects of culture differing from group to group may, under defined conditions, be regarded as having definite probabilities, p_1, of occurrence.

The diversity of the community may be regarded as equivalent to the rather vague ideas that inevitably arise from comparison of more complex and less complex cultures. In a given case, a gradual transformation in every cultural element studied merologically is conceivable, without the holological value of the complexity being changed.

It is more difficult to find an analogy to the concept of the biomass. The biomass is, however, particularly important, because it alone in the biological community has causal continuity. It is clear that the population composing a human society is involved in the analogy. Moreover, as in a biological community, causal continuity is not dependent on genetic continuity. There is also a continuity of culture that is historically determinative, and so behaves like the biomass. If we regard each individual as handing down cultural tradition to the next generation in a constant population, the total quantity Q of the culture at any time may be regarded as the product of the number of individuals N and an intensity factor I.

This concept appears artificial when viewed from the standpoint of a single culture, but may have an application when cultural contact has been established. There is a difficulty, however, that there is obviously also a quantity analogous to the partition coefficient involved here. This quantity M is vectorial, however, and may be taken as unity when no barrier exists. Thus in the suburbs of Ootacamund, the existence of numerous suburban villas, built by retired Indian civil servants of British birth, among rhododendrons and gorse bushes, has no effect on the architecture of the Todas ($M_{BT} \rightarrow 0$). But in Portuguese India, where a deliberate effort was made to reduce the partition coefficient, the presence of the Portuguese had an extraordinary effect on Hindu temple architecture. Considering the *architectural* tradition of this region ($M_{PH} \rightarrow 1$), since the Hindu population was much larger then the Portuguese, it is clear that the intensity factor was much greater in the Portuguese than in the Hindu population, owing in part to military and commercial prestige. But if the *religious* tradition be considered, this is not necessarily true, because the temple plan, as far as can be ascertained, is still fundamentally Hindu. It is probable that the function involved in cultural contact is not linear on N, but presumably, in the direction A to B, for some particular phase of culture, the effect of A on B has the form $f\left(\frac{N_A I_A}{N_B I_B} M_{AB}\right)$ per unit I_B.

It is suggested that the problems of history and archaeology can be reformulated in terms of general variables Q, I, C, M, and p_1.

The theory might be developed in terms of alternative sets of differential equations. At present, the observational data are in general nonmetric. It is not inconceivable, however, that in a large number of cases the deductions from possible theoretical alternatives can be compared in the form of relationships of *less than* or *greater than*, or geometrically in the topological form of inside or outside, which can be checked with the observational data. Relationships of this sort may give an opportunity to introduce a quantified theory into nonmetric sciences.

SECTION OF BIOLOGY

MARCH 13, 1943

Conference on *"Parasitic Diseases and American Participation in the War."*

The Section of Biology held a Conference on "Parasitic Diseases and American Participation in the War," as the third in the series for the Academic Year of 1942–1943. Doctor Horace W. Stunkard, New York University, New York, N. Y., President of the Academy, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by Horace W. Stunkard.

"Current and Post War Problems Associated with Human Protozoan Diseases," by Lowell T. Coggeshall, School of Public Health, University of Michigan.

"Changed Viewpoints on Helminthic Disease: World War I vs. World War II," by Norman R. Stoll, Rockefeller Institute.

"Arthropod Vectors of Animal Parasites of Primary Importance in the Present War," by Robert Matheson, Cornell University.

"Clinical Features of Parasitic Diseases and their Consideration in Military and Naval Operations," by Thomas T. Mackie (Lt. Colonel), Army Medical School, Washington, D. C.

NEW MEMBERS

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Lindboe, S. R., Microphysics and Ecology of the Primates, Los Angeles, Calif.

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Holmes, Chauncey D., Ph.D., Geology, Assistant Professor, University of Missouri, Columbia, Missouri.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 5 MAY, 1943 No. 7

SECTION OF GEOLOGY AND MINERALOGY[1]

APRIL 5, 1943

PROFESSOR WILLIAM A. LYNCH, Department of Physics, Fordham University, New York, N. Y.: *Deep-focus Earthquakes.* (This lecture was illustrated by lantern slides.)

INTRODUCTION

The subject of deep-focus earthquakes is introduced best by a brief consideration of normal or shallow earthquakes, which are simpler physically and seismologically. With information about normal earthquakes as a background, we can proceed to the characteristics of the deep shocks and complete the discussion by pointing out the importance of these quakes to physical and geological theory.

NORMAL EARTHQUAKES

Normal earthquakes occur at depths ranging from a few kilometers to about 50 km below the surface of the earth, the average often being taken at 25 km. California shocks are very shallow, while those in Chile that are classed as normal are actually at about 50 km. Shallow earthquakes are characterized by an intense disturbance close to the epicenter (the point on the earth's surface vertically above the origin of the dis-

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[1] The Section of Psychology met April 19, 1943. Doctor David Wechsler gave an address entitled, "Psychological Test Screening for the Emotionally Inadequate," and will submit an abstract later.

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turbance, or focus), the intensity diminishing as the distance increases from the epicenter. The probable immediate cause of the majority of normal earthquakes is slipping along a fault-plane, the earth rebounding elastically as the rupture along the plane occurs. In the San Francisco earthquake of 1906, displacements had been set up in opposite directions on either side of the San Andreas fault at distances up to five miles from the fault before the rupture occurred.

When such a disturbance originates in the elastic earth, waves of several different types travel outward with velocities determined by the kind of wave and the properties of the medium. The waves that travel through the body of the earth are of two types: compression-rarefaction waves, commonly called longitudinal, and distortional waves, commonly called transverse. The vibrations of the medium when a longitudinal wave passes are back and forth in the same direction in which the wave travels; sound waves are longitudinal. The vibrations of the medium are perpendicular to the direction of advance when a transverse wave passes; waves on a stretched string that is struck or plucked are transverse. The longitudinal wave travels faster through the earth than the transverse although both follow the same path between two points, say from focus to seismic station. As the depth increases, the velocity of each type of wave increases; hence the waves seek the greater depths and follow a curved path between the two points in order that the time of travel may be a minimum. The result is that the ray paths are concave upward. At the station, the longitudinal or P wave arrives first, followed by the transverse or S wave; the time interval between their arrivals is a measure of the distance from epicenter to station along the surface of the earth. Travel time curves have been plotted for these phases, that relate the S–P interval to the epicentral distance.

Surface waves are also generated by the disturbance. They travel at constant speed over the surface of the earth and penetrate only slightly below the surface. They are likewise of two types: the Love waves and the Rayleigh waves. The former are transverse waves that are confined largely to the surface layer; the particles of the medium vibrate horizontally only, as the waves pass. They travel more slowly than the S waves and are of greater wavelength. The Rayleigh waves are similar to water waves since the particles of the medium oscillate in vertical elliptical paths; the vibrations have no horizontal transverse component. These waves travel more slowly than the Love waves, have long wavelength, and die out exponentially with depth.

DETECTION OF EARTHQUAKE WAVES

A seismic station detects these different waves by seismometers, which are usually pendulums mounted to respond to vertical motion or to horizontal motion. The P wave disturbs the vertical pendulum more than the horizontal because it pushes upward or pulls downward at the earth's surface as it emerges. The S wave on the other hand affects the horizontal pendulum more than the vertical because it shakes the surface from side to side. A well-equipped station has at least one three-component seismograph, consisting of a vertical seismometer and two horizontal seismometers, one mounted to detect east-west motion, the other north-south. When an earthquake occurs, the seismologist examines the vertical record for the onset of the P wave and the horizontals for the S; he thus obtains the S–P interval and the epicentral distance. If all three components record P clearly, he can tell the direction from which the shock arrived.

The Love waves consist of horizontal vibrations only and do not disturb the vertical pendulum. The Rayleigh waves have both vertical and horizontal vibrations and thus record on the vertical and horizontal seismometers. The three records serve to distinguish between Love waves and Rayleigh waves and to verify the direction of arrival of the disturbance. The times of arrival of Love and Rayleigh waves check the epicentral distance.

REFLECTED WAVES

The P and S waves suffer reflections just as do other forms of wave motion. For example, the vertical seismometer records a longitudinal wave following the first P wave, that has been reflected by the surface of the earth at a point midway between the epicenter and the station. It is known as PP; that is, a P wave that has been reflected once at the earth's surface. Similarly the horizontal seismometer records a transverse wave after the first S wave, that has followed the same path as PP; it is called SS. Earthquake waves have an additional property not possessed by sound and light; they may be transformed at the time of reflection. A longitudinal wave may be reflected as a transverse wave; it is then designated as PS. Similarly a transverse wave reflected as a longitudinal wave is called SP. Other possibilities arise such as PPS and SPS.

Suppose an earthquake occurs at a distance of 6000 km. We should expect to identify these phases on the records in the following order: P, PP, PPP, S, PS, SS, SSS, the Love waves and finally the Rayleigh waves.

DEEP-FOCUS EARTHQUAKES

We are now ready to turn to deep-focus earthquakes. They are characterized by nearly uniform intensity over a large area in the vicinity of the epicenter. This becomes clear when we think of a focus at a depth of 200 km. A point on the surface of the earth 100 km. from the epicenter is only about 25 km. farther from the focus than the epicenter itself. A severe shock at this depth would affect a very large area, but the intensity near the epicenter would be small in comparison with that of a shallow shock of the same magnitude.

The records of deep-focus earthquakes differ from those of normal earthquakes in two important ways: the surface waves are weak or entirely absent; new phases make their appearance. The first property can be accounted for readily in terms of a reciprocal theorem in the mechanics of vibrations. In general, this states that if a variable force applied at one point in a system produces a certain effect at a second point, and is then transferred to apply at the second point, it will produce the same effect at the first point. We recall that surface waves in shallow quakes die out rapidly with depth; hence a disturbance at depth will produce little effect of this kind at the surface. Jeffreys has calculated the magnitude of the result and states that the records of quakes at depths greater than 150 km. should be practically free of surface waves. Very often a glance at the records is sufficient to establish the fact of abnormal depth.

The second characteristic requires more consideration. Suppose an earthquake occurs at a depth of 300 km. It sends out P and S waves that travel to a station by the path of shortest time as in normal shocks. It also sends out waves that are received as PP and SS after a single reflection at a point less than midway between epicenter and station. A third longitudinal wave leaves the focus in a direction such that it strikes the surface near the epicenter, is reflected and continues on to the station, where it arrives after P but before PP; this phase is designated pP, and is found regularly on the records of shocks that have abnormal depth. A third transverse wave behaves in the same manner, arrives after S but before SS and is called sS. The time interval pP–P, between the arrival of P and that of pP, increases rapidly as the depth of focus increases but changes only slowly with distance. It is the most important interval that we have for determining depth. The interval sS–S is a check on the results obtained by the use of pP–P.

Other phases occur such as sP and pS. The former starts as a transverse wave, is reflected at the surface near the epicenter, is transformed by the reflection and completes its journey to the seismic station as a longitudinal wave; the latter behaves in just the opposite fashion. Still others exist, for example sPP, sPS, sSP, pPS and so on. It is evident that the seismograms of deep shocks may be exceedingly complex.

To illustrate the magnitude of the time intervals involved, we may take the following from the tables of Gutenberg and Richter:

TABLE 1

DIFFERENCES IN TRAVEL TIMES, pP–P, IN MIN. : SEC.

Epicentral distance in degrees	Depth of focus in kilometers						
	100	200	300	400	500	600	700
50	0 : 26	0 : 45	1 : 05	1 : 21	1 : 36	1 : 50	2 : 03
60	0 : 25	0 : 47	1 : 07	1 : 25	1 : 41	1 : 55	2 : 08
70	0 : 25	0 : 48	1 : 10	1 : 29	1 : 47	2 : 02	2 : 15

TABLE 2

DIFFERENCES IN TRAVEL TIMES, sS–S, IN MIN. : SEC.

Epicentral distance in degrees	Depth of focus in kilometers						
	100	200	300	400	500	600	700
50	0 : 35	1 : 21	1 : 54	2 : 23	2 : 51	3 : 17	3 : 41
60	0 : 44	1 : 24	1 : 58	2 : 30	2 : 59	3 : 26	4 : 03
70	0 : 41	1 : 22	2 : 00	2 : 35	3 : 06	3 : 36	4 : 03

The seismograms of deep-focus earthquakes are clear and the time intervals are easily read as the phases have very sharp beginnings. This is particularly true of P, pP, S and sS, those phases that are most important. Distance is determined by the interval S–P but special travel time curves must be drawn for each depth. Gutenberg and Richter have made the calculations for depths from 100 km. to 800 km. for all the phases that are commonly recorded. In the seismological observatory we use at least nine charts of travel time curves: one for normal shocks and the others for the different depths from 100 km. to 800 km. by steps of 100 km.

DEEP-FOCUS EARTHQUAKES AND THE INTERIOR OF THE EARTH

The realization that deep-focus earthquakes do occur has opened up new avenues of approach to the study of the interior of the earth.

Phases that are recorded in normal earthquakes among the surface waves are difficult to isolate and identify because of the continued and vigorous disturbance in that part of the record. This is particularly true of certain phases reflected from the earth's core. The seismologist believes that about midway to the center of the earth there is a very definite discontinuity, a change in structure so marked that earthquake waves are reflected from it and are strongly refracted if they pass the boundary into the lower medium. Longitudinal waves are changed in velocity from about 13.8 km./sec. to 7.5 km./sec. as they enter the core. Transverse waves, however, appear not to pass through the core. This has led to the belief that the core is fluid, because it permits the passage of longitudinal waves but not of transverse waves. A longitudinal wave that strikes the core is partly reflected and partly transmitted. Let us follow the reflected phase. It travels a curved path up to the surface and is detected by a vertical seismometer as a P phase; but it arrives much later than the initial P, at the shorter epicentral distances. The time interval is about 4 minutes at 2000 km. This phase is called PcP, the c indicating that reflection occurs at the core. In normal earthquakes PcP is not very prominent and may be lost in the disturbance due to S on the record at small distances from the epicenter, and that due to the reflected waves PP and PPP at greater distances. In deep-focus quakes, it is often sharp and well-defined on the record. More important than PcP is the transverse wave, ScS, that behaves in a similar manner. It is more energetic, however, because transverse vibrations do not pass through the core and a larger fraction of the shear energy is reflected. It is recorded on the horizontal seismometer. At an epicentral distance of 2000 km., it arrives about 11 minutes after P in the midst of the surface waves if the earthquake is shallow, but in an undisturbed portion of the record if the shock is deep. The deep-focus records give us the opportunity of observing PcP and ScS and other waves similar to them such as PcS, ScP, pPcP and sScS. If these reflected waves are correctly identified and their travel times determined accurately, the distance to the core can be obtained. Jeffreys states that if the travel time of PcP is known to a second, the depth to the core can be found to within 7 km.; and if ScS is known to a second, the depth can be found to 4 km. This corresponds to a high degree of precision as the distance involved is about 3000 km. In order that the measurement may be successful, it is necessary to locate the epicenter of the deep-focus shock precisely and to find its depth exactly. It is difficult to do both of these.

Let us follow that part of the longitudinal wave that entered the core. It is refracted as it enters, follows a curved path through the core, is refracted again as it emerges, and follows a final curved path to the surface. The symbol used for this phase is PKP or P′. The K indicates that this leg of the path is through the core, the disturbance travelling as a longitudinal wave. An S wave must be transformed into a longitudinal wave to be transmitted through the core. We find that phases such as SKP and SKS emerge on the far side. The former completes the final stage of its journey as a longitudinal while the latter reaches the surface as a transverse wave. These and still more complex combinations are found on the records of earthquakes at large epicentral distances. When the shocks are deep-seated, many more phases are present, such as pP′, sSKS, pSKP and so on. All of these are more sharply defined on the records of deep earthquakes than the corresponding phases of normal shocks. Their study gives us further information about the boundary of the core and the structure of its interior, for these waves are the most direct probes that we have of the interior of the earth. A complete interpretation of the seismograms of an earthquake at an epicentral distance of about 145° would give us a very detailed description of the properties of the interior of the earth.

MORE ABOUT THE INTERIOR OF THE EARTH

It is clear from the above statements that the seismologist is convinced that earthquakes occur at depths far below the surface. The estimated depth of the deepest thus far recorded is about 750 km. Conditions in this region must be entirely different from those where shallow earthquakes occur. However, the records of both normal and deep shocks are fundamentally the same. The differences are accounted for, not in terms of the physical properties of the interior at different levels, but largely in terms of the geometrical and mechanical changes produced by a change in level of the origin of the disturbance. The P phases and the S phases are vigorous and pronounced on the records of deep shocks; hence energy must have been stored in the forms of compressive strain and of torsional strain or shear. But is is believed by the isostasists that below about 100 km. the interior is in a plastic condition and inequalities of stress are relieved by slow plastic flow. An earthquake is a sudden fracture following a rapid building up of stress that the slow process of plastic flow cannot relieve. The plastic material can store shear energy in spite of its fluid properties because of the high viscosity at great depths. Internal friction opposes relative motion of different layers until the limit of restraint is reached; and then a rupture occurs. Deep earthquakes occur periodically in the Hindu Kush Mountains. Instrumental observation places all of them at the same epicenter and depth, about 230 km. More than 40 have been recorded since 1907, many of them violent. The method by which the energy is renewed and stored is not clear as yet; but the mechanism of the earthquake itself seems to be the same as that of a normal shock, a slipping along a fault-plane.

Even though the material at depth in the mantle may be in a plastic condition, it can transmit transverse earthquake waves, for it behaves toward rapid vibrations as though it were an ordinary solid. Tar and sealing wax flow when left unsupported for a long time and yet vibrate transversely when struck. The element of time must be taken into account when investigating phenomena of this type, and further discussion of this aspect of the subject would take us too far afield.

DISTRIBUTION OF DEEP-FOCUS EARTHQUAKES

A second problem of major proportions for the geologist concerns the distribution of deep-focus earthquakes: A great belt encircling the Pacific Ocean is the locus of a great majority of the abnormal shocks. Two large gaps exist in this belt, however: one from Central Mexico to Alaska in which deep shocks are unknown, and the other from the vicinity of New Zealand to South America where seismic activity in general is relatively slight.

Along the belt where deep earthquakes occur, those of greatest depth are on the inland side, away from the Pacific. Shocks of intermediate depth are nearer the ocean, while the shallow earthquakes are along the coast or out in the sea. This type of distribution is prominent in South America. Epicenters of shocks from 600 km. to 660 km. depth lie east of the Andes with which they do not seem to be connected; those from 100 km. to 290 km. are mostly in the Andes; while the shallow quakes are close to the coast. Gutenberg and Richter state that they have found no South American shocks at depths between 290 km. and 600 km. The distribution of earthquakes at depth is complicated in the Japanese Islands and in the Dutch East Indies, but the general rule seems to hold throughout. Gutenberg is led to the conclusion that the deep-focus belt marks the boundary between the two types of crustal structure, continental and Pacific.

The only zone of deep earthquakes outside of the circum-Pacific belt is that extending across Central Asia into Europe north of the Mediterranean. The greatest depth thus far recorded here is about 300 km. The Hindu Kush Mountains are in the zone, and the other seismic regions of the Himalayas. This zone also includes Rumania where shocks occur periodically at about 150 km., much as they do in the Hindu Kush. The most recent was the violent earthquake of November 1940.

CONCLUSION

An attempt has been made to summarize the principles of seismology with special reference to deep-focus earthquakes, and to point out some of the problems that have arisen as a result of the discovery that earthquakes do occur at great depths. Continued investigation of deep earthquakes is bound to lead to a better understanding of the interior of the earth. It is hoped that what has been presented here will aid those who wish to read further in the subject.

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SECTION OF BIOLOGY

April 12, 1943

Doctor Charles M. Breder, Jr., The New York Zoological Society, New York, N. Y.: *Problems in the Behavior and Evolution of a Species of Blind Cave Fish.* (This lecture was illustrated by motion pictures, lantern slides and living specimens.)

There are only twenty species of definitely blind and whitish cave fishes known, which number may be reducible to sixteen if broader specific definitions are employed. These are from widely separated localities. In addition there is about an equal number of forms with more or less defective eyes which are associated with caves or other cryptic types of environment,[10] exclusive of blind fishes of the oceanic depths.

Relatively few of the abyssal fishes are blind, and in this special situation optical stimulation is far from absent, since many species of abyssal fishes and invertebrates possess luminous organs which are evidently of considerable biological significance. On the other hand, the fresh water caves of the world present an optical void, for no light-giving structures have been developed by the aquatic inhabitants and, save for a relatively few rare items such as phosphorescent bacteria, cave waters are without light stimulation.

The sixteen genera of entirely blind cave fishes proper represent seven orders, distributed through nine families. The others, with more or less defective eyes, of which there are about an equal number of species in about twenty genera are nearly all more or less related to the true cave fishes, but grade off insensibly to normal types. However, in all of these groups not one blind cave fish is known to be genetically connected with its nearest non-blind surface relative, excluding only the case which is the subject of the present remarks.

In 1936 a new cave fish was described from Mexico by Hubbs and Innes. This fish they called *Anoptichthys jordani*, new species, new genus, which was described as differing from its nearest relative *Astyanax mexicanus* (Philippi) only in the absence of pigmentation and the absence of an eye.[11] Strictly taxonomic problems involved in this material will not be discussed at this time. Since the original discovery there have been three additional visits to this cave, which is known as La Cueva Chica, by American students; Dr. Myron Gordon in 1939, the New York

Zoological Society expedition[8] in 1940, and Mr. B. Dontzin,[2] for the Society in 1942. Cueva de los Sabinos, another cave about fifteen miles distant has yielded an undescribed form, slightly differing from the first, which was also found by one of Dr. Hubbs' collectors. Mr. Dontzin visited this other cave and brought back the first living specimens which are now under experimental study.*

These fishes differ from other known cave fish in a number of respects which make them especially suitable for experimental study and which led to the Society expeditions of 1940 and 1942. Some of the more striking of these features follow:

1. The La Cueva Chica fish apparently differs from the surface form only in lacking visual organs and pigmentation.
2. The group (Family Characidae) is one in which it would hardly be expected to find a blind offshoot, for they are types largely dependent on their eyes. Most other cave fishes have been derived from groups that already have marked tactile or other sensory developments in addition to sight.
3. They thrive well in ordinary warm aquaria of standing water. The caves which they inhabit are warm, an unusual circumstance, because caves are normally cold even in the tropics.

It was with these features in mind that the 1940 expedition was undertaken, in order that some understanding of the ecological background could be obtained for aid in connection with projected laboratory experiments. Although there was some slight variation in the fishes previously collected, their closely bred descendants in aquaria showed no greater variation. It remained for this party to uncover the fact that there was a complete intergradation from the outside river population to the fully blind and white cave form, which condition ran as a gradient from the river connection.[1] Many of these were brought back in a living condition and now form the basis of breeding experiments. It is too soon to say anything definitive regarding the genetics of the situation but it is evident that there are a considerable number of factors involved, the genotypic expression of which produces a population showing about every degree of intermediacy that could be expected, including all degrees of pigmentation, reduction of eye size and loss of parts of the eye, which features lack complete correlation. There are some specimens lacking pigment but having eyes, pigmented individuals without eyes, and even some unilaterally blind specimens.[1] It appears that the first generation of a cross between a white blind fish and an eyed pigmented fish all resemble the latter parent, but there is a peculiar behavior item to be dealt with later that may vitiate this apparent result of tank breeding.

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*The current laboratory work is being carried on in the Department of Animal Behavior of The American Museum of Natural History.

There is still another complicating item to be considered. The first pool in the cave that is encountered on entry has had four collections taken from it over a period of six years. The first two obtained only the blind white fish; the third, blind plus some intermediate fishes and a few with normal eyes; while the fourth obtained still more eyed intermediates and eyed forms with full pigmentation, a type not present in the first three. The situation thus stands with an increasing gradient in eye and pigment as the river is approached and an apparent similar gradient in time at the point most remote from the river. It would seem that here are two basic possibilities in regard to speciation.

1. That these populations have always been in contact and that various waves of migration inward by river fish give fluctuations over the years in the phenotypes found, some of which are truly invaders and some of which are the result of eyed and blind crosses, with a primary zone of intermediates.
2. That these populations have actually been separated by some barrier that has been recently removed and that the intermediates are the "hybrid" product of the merging populations, perhaps moving toward a swamping out of the smaller blind population, with a secondary zone of intermediates.[2]

Before one can attempt to evaluate these possibilities it is desirable to know something of the differential behavior of these fishes, which, as will be shown, has a considerable bearing on the possibilities involved. In order to undertake such experiments intelligently one must first have some understanding of the sensory characteristics of the river and cave fishes.

The eye of the river fish is the dominating sense organ as is indicated both by the behavior of the fish in its social life and feeding reactions, the structure of the eye and of the relatively great development of the optic lobes. The remnant eye of the blind form is a tiny bean-shaped capsule at the base of the eye socket composed of the heavily pigmented choroid. Within this are a few scattered retinal elements and a much reduced optic nerve.[9] Nevertheless, these fish are light-sensitive to a slight degree, as has been shown by work with a gradient trough. Extirpation of this capsule makes them completely indifferent to light, whereas formerly they were slightly but significantly negative to light. Large old specimens with a greater mass of tissue overlaying. the capsule are less negative than small ones without such a depth of tissue covering the capsule. Specimens with deformed eyes, of whatever size, are not light negative in this sense, but if given the opportunity will spend much time in the shelter of a rock or other object, peering out into the lighted area. These forms are easily identified in the living state by feeding experiments. Those which form a retinal image will strike accurately at falling particles of food very much after the manner of a trout. Those which cannot, on the slightest disturbance, will go to the bottom and perform circling movements of much vigor until they find the food. Those with an exposed retina will "jump" when a shadow is passed over them while those properly blind will not. This refers to fish that have been kept for long in light during the feeding tests.[3, 4, 5,]

Here again a time element enters. All fish brought freshly from the cave act like the blind fish in regard to feeding. This has been checked twice, all forms from the cave performing circling movements for food finding for at least a couple of months and then those with structurally good eyes apparently learning the meaning of a retinal image and taking on the feeding habits of the river fish. These forms at all times, from the first, "jump" at the passage of a shadow, like those with merely an exposed retina.

Simple observation indicates that these fish are very sensitive to mechanical disturbance and probably true sound waves. One of the easiest ways to catch them in the cave was to drop small objects in the water, about which they would gather even from a considerable distance. In deep water the gathering would be near the surface where the splash was made and not on the bottom. In captivity they could be "called" from inside an artificial cave by waving a finger in the outside water. Anatomical examination shows that the ears are degenerating but not as markedly as in the case of the eyes.[12] There is no evident hypertrophy of the lateral line system, but work on both these items is not yet sufficiently advanced to be able to say anything definitive. There is, however, no evident distance perception of obstacles as reported for other species of cave fishes. Placed in a strange environment they freely "stumble" into obstructions. In a short time they seem to learn the location of such objects by trial and error and from then on avoid them. Changing the position of a rock, however, will force the fish to learn all over again. A changeable obstacle such as the dangling legs of a large frog always gives them trouble. In an outdoor pool which was inhabited by several bullfrogs, some would collide with the feet of the latter continually, with the result that they would charge off at random and take some time to find themselves, again colliding with all manner of permanent and familiar fixtures.[6] While they do sometimes strike one another, it seems that the swimming activity of the other fish gives them some warning to turn.

The great number of replacement scales found on the wild fish indicates that they normally lose many more scales than do fishes in general. Many of the larger fish have very few of their original scales. These losses presumably occur when, for some reason, they are frightened out of their own territory or perhaps washed out when there are freshets.[1]

The chemical senses are apparently normally developed in these fish, but do not seem to be hypertrophied. They show positive and negative reactions similar to those of various surface fishes.[7] Food is no problem to these fish because of the presence of insectiverous bats. Examination of the stomach contents showed that the food consisted entirely of bat droppings and the eggs of each other. Bat droppings are continually supplied and contain a rich fauna of some macroscopic nematode. This is supplemented by an occasional dead bat. That the eyed river fish are able to survive in this same situation is evident from the condition of those found in the cave and the fact that they live well under not dissimilar conditions in captivity. Their food requirements are simple and there is as yet no reason to suppose that there has been any improvement in their chemical sensory development. Histological examination shows no significant structural modifications in the fish from either cave.[7]

Thus, as far as present evidence goes, at least, we have a case of an intrusion into a "simpler" environment with a loss of sensory refinements but with no evident substitute structural developments. It suggests rather an analogy to a trematode living in a mammalian gut than to what has been reported for other cave fishes. Food is extremely abundant, predaceous enemies are evidently absent and reproduction would seem to be the prime concern. Before considering that, however, a brief review of the social aspects of blindness in these fishes should be made.

The normal-eyed river fish are strongly gregarious and when there is no special external stimulus they rest in a closely compact school all facing the same way, which is apparently disrupted only by fright, food or reproductive activity.[3] This gregarious habit, so far as known, is inspired by visual perception. The blind fish do not form schools but wander continually at random, as would be expected. This wandering movement is apparently kept up at practically all times and has been shown experimentally to be truly at random and without reference to other individuals. Incidentally, the blind fish eat much more than the eyed ones, which may have to do with the extra energy expended in their continual wandering. Blind and eyed fish placed together for the purpose of cross breeding present a bothersome problem. The eyed member attempts to follow the blind one in its aimless wandering. This very often results in disaster for one or the other, because:

1. The eyed fish may become emaciated and eventually die.
2. The eyed fish may become erratic in behavior. One actually took to spinning on its snout at one end of the tank, but recovered after removal.
3. Most likely the eyed will attack the blind and destroy it.
4. An accommodation may be made and the eyed fish subsequently pay little heed to the wandering of the blind.

This kind of behavior in light may well result in the survival of eyed forms only from a mixed spawning, which could easily lead to an erroneous genetic interpretation. This feature has not been adequately checked as yet.

Reproduction in the river fish is of the simplest sort; the pairing and courting consists of chasing by the male, circling by both fishes and finally the ejection of eggs and sperm while the fishes take a side-to-side position. This item of behavior is evidently entirely visual. Blind fish, apparently little aware of each other, when in the proper physiological state, increase the rate of their "random" movements far beyond the normal speed. They are not so apt as usual to turn aside from one another and collisions are more frequent. In short they do exactly what one would expect a blind fish to do. They circle and gyrate about as the crucial time approaches but continually "lose" one another. In one case a female cutting small circles at one end of the tank discharged her spawn while the male was wildly dashing up and down the glass at the far end. Nevertheless the eggs hatched, for in spite of this distance they were fertilized. It might be objected that this was a special case in a small aquarium and would not take place in nature. It must be called to mind that many of these fishes are living in small calcium cups in the flow-stone of the cave which are not materially larger than a moderate sized aquarium. Further, due to the nature of such caves there are countless ways in which groups may be trapped in small quarters and it is possible that here only is successful spawning attained.

The normal river fish are very positively rheotropic and will follow up the slightest flow. Since the water leaves this cave to empty into the river this is one likely cause of original entry. Further, it has been shown that as slight a differential as 1.5° F. will cause eyed and blind alike to enter or leave an artificial cave independent of the presence or absence of light outside, and subsequent work indicates this reaction may follow on an even smaller thermal difference.[5] Since frequently the cave waters are somewhat higher in temperature than the river this is certainly another cause of entry.

With a cave constituted as is La Cueva Chica, with outflowing water heated from below and warmer than the surface water for at least part of the year, and fish impelled to enter dark places and move upstream it is easy to see that under such influences the initial behavior of the fishes should lead to cave entry. Once inside they would have difficulty getting out, for when far enough in to lose the guiding influence of daylight they would have to reverse their normal rheotropism to come out in the open again. The onset of nightfall would also cause many to lose their return route. In this connection it should be remembered that nocturnal animals, on a clouded night, are voluntarily active under conditions similar to those permanent to a cave.

These river fish have a widespread constitutional variation in eye size. Locally it is possible that this might be interpreted as a genetic contamination from the cave population, but elsewhere this is not likely to be the case. Dr. Hubbs assures me on the basis of taxonomic measurements throughout the range of these fishes that it is widespread.

As the initial cave population would presumably be relatively small, it is not surprising that eye defects would be preserved and spread. The large amount of predation in the river would certainly not permit the establishment of a serious eye defect outside the cave. The gradient in the cave population as found would become clear on such a hypothesis, as there would be a continual infiltration of the river fish into the cave on a basis of their known tropisms. Movement back to the river of even the optically sound elements of the population would surely result in them being lost before they could learn the significance of a retinal image.

These are all, of course, negative elements in that they simply permit the continuation of defects but in themselves do nothing to suggest the selective elimination of the eyed fish within the cave. It may be that there is such a process in progress based on a possible psychic difference between a fish with good eyes living in darkness and one without such optical equipment. However, blinded river fish behave in a manner indistinguishable from those born blind.[7] The new form from

Cueva de los Sabinos, which has yielded no intermediates, differs principally from the other in the further progress in the same direction. These fish are more nearly transparent and consequently more pinkish in color and the internal anatomy is more evident. The choroid capsule of the other form is represented by some scattered pigment, but there are no retinal elements and the optic nerve is absent.[6] The shape of the mouth is slightly broader, the only evident difference so far noted not apparently clearly associated with a reduction of refinements. These fish also have a distinctly shallower nasal pit, the possible significance of which is not clear at this writing. Obviously it will be of considerable interest to determine if this population is homozygous for eye defect and associated items.

Whether the Cueva de los Sabinos stock is in contact with another form, eyed or blind, is a question that calls for more field work, as the cave is large and not thoroughly explored. In the meantime, laboratory experiments are under way to determine what behavior elements differentiate it from the La Cueva Chica material. So far it has been shown that there is no light sensitivity in this form, as would be expected with the elimination of the eye capsule.[6]

Given such a river species, with the described habits and carrying an eye irregularity, this kind of evolution could have conceivably taken place many times and it may well be that these two blind populations are independent developments from the same basic stock and differ only in the accidental assortment of genotypes that gave rise to them. Evidently the very marked difference in the behavior patterns of the eyed river fish and the blind forms is directly referable to the presence or absence of vision as has been shown by the immediate change to the "normal" blind behavior of freshly blinded specimens.

The question of the establishment of an apparently pure stock in Cueva de los Sabinos would seem to call for some as yet unknown selective process. The populations are no doubt comparatively small with breeding confined to limited colonies but, since the genic constitution is surely complex, it is hard to imagine that on such basis alone the intermediates could be fully eliminated.

An interesting speculation on the possible mathematics of the genetic reduction of other than blind white forms coupled with the repeated separation of small groups within a cave is discussed by Hubbs.[10] Actually, the geological separations found in La Cueva Chica could supply such a physical condition, as already noted, and the observed gradient from the river could be supposed to rest on such a basis. In any case it should be evident that this material provides inviting problems both for the laboratory and the field.

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SECTION OF ANTHROPOLOGY

APRIL 26, 1943

DOCTOR MARGARET MEAD, Associate Curator of Anthropology, The American Museum of Natural History, New York, N. Y.; Executive Secretary, National Research Council, Washington, D. C.: *Anthropological Approach to Dietary Problems.*

During the last year this section of the Academy has been devoted to discussions of the abstract contribution to be made to cultural anthropology from other sciences. There has furthermore been an attempt to relate the discussions to problems of immediate practical interest of war and post-war operations. Such an inquiry leads naturally to a consideration of the way in which the development of better abstractions will make it possible to employ coordinately the techniques of one or more scientific disciplines besides anthropology to the solution of a problem.

Diet is one of the fields which is particularly crucial to the war and the future successful organization of society on a world scale. Furthermore, it is a field where the findings from the natural sciences are susceptible to an integrated cross-disciplinary approach at two points: (1) the findings of soil chemistry, food chemistry, and the physiological study of nutritional status, and (2) the findings of psychosomatic studies, particularly in those disease pictures which involve the gastro-intestinal tract. At perhaps no other point of contemporary scientific attack on the problem of social change do we have available so many of the necessary techniques, which in the matter of diet include: techniques for assaying and supplementing the contribution of the physical environment and improving the stock of domestic plants and animals; techniques for assaying the nutritional content of the plant stuffs and animal products; methods of describing and analyzing the culture that patterns the way in which man uses this physical environment, determines his selection of foods and his methods of preparation and consumption; methods of studying the developing child so as to understand the way in which hunger is transformed into appetite by the systematic inculcation of attitudes toward food and own body; techniques for assaying the use which the human body makes of the food which is consumed; methods derived from clinical psychology and psychiatry for exploring the dynamics of food acceptances, rejections and symbolisms through the study of the psychopathology of gastro-intestinal diseases.

The satisfactory integration of all of these disciplines depends upon (1) an adequate conceptual scheme which will place each contribution exactly in the total context and (2) the state of the techniques in each of the cooperating fields.

In considering the techniques, it is valuable also to distinguish between the state of development of analytic techniques at a merely descriptive level, and the development of methods of using these techniques for controlled change. For example, soil analysis is very far along in the determination of the chemical content of the soil, quite far along in methods of supplementing or correcting that content for deficiencies or stepping up potentialities so that there will be an improved crop yield, when yield is considered in terms of volume per acre or when such factors as viability under adverse weather conditions, differential growth of grain and straw, storage quality of grain, etc., are considered. In the cases where alterations in the content of the soil are made to alter the quality of the meat from cattle pastured or fed from the products of that soil, there has also been a considerable technical development, with the emphasis, however, on quality values in the meat which are not necessarily related to nutrition. But in the field of relating soil composition and problems of supplementing or exploiting the qualities of the soil so as to produce food, either plant food directly or animal foods through the feeding of livestock, so that the food will have the highest nutritional yield, there is as yet only a beginning. The techniques are available but the details of application have to be worked out. When it comes to a question of adjusting the soil potentialities, as expressed in plant foods and in livestock feeding and poultry raising, so as to produce a completely adequate diet from a given geographical region, the problem has hardly been tackled and the techniques for a coordination of the relationship between the soil, the methods of soil supplementation and the nutritional content of the food so produced are in their infancy. But what is lacking is not a technique for assaying the food composition of carrots grown on a certain kind of soil, nor do we lack quick methods of mapping soil composition. The mere recognition by those who are planning a scientific attack on the problem of diet of the importance of integrating these existing techniques will be sufficient to bring them all into a coordinated relationship.

At this level however, there are a large number of cultural factors to be considered, varying from traditional patterns of land ownership, inheritance, tenancy, etc., through traditional methods of planting, cultivation, rotation, stock feeding, choice of money crops, methods of marketing, export and import of foods into a given area, etc., which must be taken into account in determining how a living population's habits of food production and gross food utilization can be fitted together with the maximum utilization of the particular composition of the local soils. Furthermore, this problem ramifies from problems of land use on a given farm, where traditional crop choices or patterns of location of the garden may have to be altered to fit the realities of soil composition, through problems of national, continental, and finally global alterations of existing food production habits so that each area yields not only that food which under existing circumstances can be produced for a profit, but that which actually makes the best use of its soil and water conditions, so that the population of each area of the world may be adequately nourished.

It is wholly probable that states of malnutrition may be produced at any time through a shift in the traditional composition of imported and locally grown foods; but it is theoretically conceivable, especially in the case of the rarer mineral constituents of the diet that the substitution of the same food, grown on soil which produces an inferior product, may throw an entire diet out of proportion. There is also a problem of balance in planning social change. To what extent should a pattern which involves alteration in basic cultural habits be based upon a combination of economic, agronomic and nutritional thinking, which it then becomes the task of the cultural anthropologist merely to implement? How much responsibility should the cultural anthropologist assume for outlining the profound repercussions which a fundamental alteration of food production habits may have on the total culture and on the character structure of those who embody that culture?

What may be said of alterations in the field of production and distribution applies with equal force to the whole problem of food selection and consumption at the household level. The scientific techniques are being rapidly developed to evaluate the results of different sorts of storage, preparation and serving procedures, and are of course already available on the subject of balance of meals over time for different age levels and different types of occupation. These will of course be altered as knowledge increases, but enough is known to reveal a profound discrepancy between current practices and the fullest utilization of available foodstuffs for the nutrition of the family.

Historically the social sciences have been looked to as a sort of super-propaganda agency; nutritionists have tended to ask from the social scientists: How do we put this across? What techniques will be most useful in getting people to drink more milk, or eat more oranges?

A complete anthropological approach will take into account: the way in which those things that are considered to be food are distinguished from those edible objects that are not regarded as food; thè development of culturally sanctioned acceptances and rejections; the way in which food and feeding situations are used as potent instrumentalities for personality development; the integration of food behavior with other types of behavior such as sex behavior; the role that food choices may play in the development of conscience, of caste and class feeling, etc. That is, between the operation of the food production pattern, itself part of the culture, and the results which can be determined by a nutritional assay of the individual, the cultural anthropologist must interpolate an understanding of the role food plays in the system of interpersonal communication between the members of a society and each newborn member. Only if the place of food in a culture is accurately described and evaluated can the anthropologist perform either of the two functions discussed above, that of implementing a process of change and that of giving a critical estimate of just what the impact of any change in food patterns will be.

If adequate insight is to be gained into the dynamics of the ordinary character structure of the culture and the role that food plays, then the anthropologist can find much help in the findings of psychosomatic medicine, which will simultaneously illuminate the more typical character structure—in regard to food—and also, in the studies of deviant individuals. Deviant characteristics can be used to interpret the more frequent types of behavior. The regular patterns of distribution of succoring and dependence between males and females, or the extent to which a meal with a climax in which the end is the reward for having eaten the beginning becomes part of the typical conscience structure of members of a given culture, can be given additional force if we have detailed case studies of the mechanisms involved.

To the extent that the anthropologist is asked to assist in bringing about purposeful change in the behavior of a people in regard to food, whether it be production, distribution or consumption, it is necessary to know the way in which the desirable changes of habits are related to the entire system of behaviors that constitute the developed cultural personality. It is not sufficient for the anthropologist to provide the techniques by which people can be persuaded to eat more bread or less meat, plant gardens, or cook their vegetables for a shorter period. Alterations in the dietary practices of a people involve alterations in their whole behavior and may disturb the whole pattern of individual responsibility, destroy the sanctions upon which parental authority is based, or alter a capacity for cooperative behavior. It is therefore the task of cultural anthropology to provide this part of the conceptual scheme within which the various natural sciences concerned with soil, food composition and nutrition, can operate with due regard for the unitary character of human culture, informed at every step of the possible consequences of the changes they wish to bring about. The simple maxim that people are inevitably better off if they have a better diet has to be qualified by a recognition that the means taken to reorganize patterns of behavior so that the better diet is possible may themselves be either constructive or destructive and outweigh in importance any dietary change short of an exterminating famine.

Anthropological techniques are already available for giving a thorough description of the integration of food in any culture, and on the basis of this description, for providing help in altering the food pattern and cautions about the results of such alteration. Less well developed, however, are the techniques for setting in motion within a culture a type of cultural behavior which is itself a form of prospective adaptation to change. It would be comparatively easy to introduce orange juice finally into the meal pattern of every American; it is more difficult to educate every American so that twenty years later whatever vitamin C carrier is recommended will be introduced in the diet, with sufficient flexibility so that orange juice may be displaced. In applying anthropology to problems set by government, the usual assumption has been that the answer was already known. It was desirable that natives should wash their clothes or use latrines or grow some supplementary vegetables, while they abandoned head hunting and other socially unacceptable practices. The food sciences themselves, however, are in a state of development and any rigid adjustment of whole cultures to present procedures or present knowledge would be most unfortunate. The problem for the anthropologist therefore is to devise methods by which an expectation that dietary patterns will change can be combined with a widespread acceptance of existing best practice.

Historically the rationalization, that is, the application of science to some area of human life which had formerly been governed by tradition, has brought about profound disorganization with exceedingly heavy consequences in human maladjustment. The necessary knowledge combined with the impulse to alter dietary practices comes at a stage when anthropology should be able to participate effectively in planned social change so as to obviate this disorganization and develop a style of procedure which may subsequently be applied to other fields as the techniques necessary become progressively available.

SECTION OF PHYSICS AND CHEMISTRY

APRIL 16 AND 17, 1943

Conference on *"Sulfonamides."*

The Section of Physics and Chemistry held a Conference on "Sulfonamides," as the fourth in the series for the Academic Year of 1942–1943. Doctor Colin M. McLeod, New York University, College of Medicine, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"A Theory of the Relation of Structure to Activity of Sulfonilamide Type Compounds"—

A. "Biological and Organic Chemical Aspects," by Richard O. Roblin, Jr., American Cyanamid Company, Stamford, Conn.

B. "Physical and Structural Aspects," by Paul H. Bell, American Cyanamid Company, Stamford, Conn.

"Relation between Chemical Structure and Physiological Disposition of Sulfonamides," by James A. Shannon, New York University, College of Medicine, New York, N. Y.

"Toxic Action of Sulfonamides," by H. B. van Dyke, the Squibb Institute for Medical Research, New Brunswick, N. J.

"Antagonists (other than *p*-aminobenzoic-acid) and Potentiators of the Sulfonamides," by Henry I. Kohn, Duke University, School of Medicine, Durham, N. C.

"The Action of Sulfonamides in the Body," by John S. Lockwood, University of Pennsylvania, School of Medicine, Philadelphia, Pa.

ANNOUNCEMENT OF PUBLICATIONS FOR 1943[1]

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution, except as qualified in the footnotes:[2]

Annals:

1. "Boundary Layer Problems of the Atmosphere and Ocean." Papers delivered at the conference by this title. (Approximately 110 pages.)
2. "Criteria of Subspecies, Species and Genera of the Vertebrates." Papers delivered at the Symposium on Speciation, joint Annual Meeting of the American Society of Mammalogists and American Society of Ichthyologists and Herpetologists, March 31–April 2, 1942. (Approximately 80 pages.)
3. "High Polymers." Papers delivered at the conference of this title. (Approximately 150 pages.)
4. "New Methods in Stellar Dynamics," by S. Chandrasekhar. Awarded an A. Cressy Morrison Prize, 1943. (Approximately 30 pages.)
5. "The Relation Between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails," by Arthur W. and Priscilla F. Pollister. Received Honorable Mention, A. Cressy Morrison Prize Competition, 1942. (Approximately 50 pages.)
6. "Studies on Fresh Water Bryozoa, XIV. The Occurrence of *Stolella indica* in North America," by Mary D. Rogick. (Approximately 16–20 pages.)
7. "Lycaenidae of the Antilles," by William P. Comstock and E. Irving Huntington. (Approximately 60 pages.)
8. "Parasitic Diseases and American Participation in the War." Papers delivered at the conference by this title. (Approximately 60–70 pages.)
9. "Color Constitution and Reactions of Dyes." Papers delivered at the conference by this title. (To be published later.)

Transactions:

Series II, Volume 5, Nos. 1–8. These papers are sent to all members of the Academy, regularly, during the academic year. (Approximately 200 pages.)

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[1] Notice of additional publications to be added to this list will be sent to members later.

[2] Active, Sustaining, Life, and Honorary Members may receive, upon request, a copy of all current numbers of the Annals.

Student and Associate Members are entitled to receive one complete monograph or up to 150 pages of smaller papers.

NEW MEMBERS

Elected April 22, 1943

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van Dyke, H. B., Ph.D., M.D., Medical Research (Pharmacology), Head of Division of Pharmacology, The Squibb Institute, New Brunswick, N. J.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 5 June, 1943 No. 8

SECTION OF GEOLOGY AND MINERALOGY[1]

May 3, 1943

Lieutenant Colonel Gerald FitzGerald, Chief, Aeronautical Chart Service, American Air Force: *Reconnaissance Mapping by Photogrammetric Methods.*[2] (This lecture was illustrated by lantern slides.)

Military operations in wartime have always affected the development of mapping techniques. It is natural, therefore, that from the time of Col. Laussedat (1849) military scientists of many countries have studied the application of photography to mapping. The Balloon Corps of the U. S. Army considered the application of aerial photographs to mapping as early as 1861 and Lt. Col. Reber of the Signal Corps actually compiled a topographic map from aerial photographs during the Spanish American War. He described a radial line method of compilation in a manual issued in 1896.

Prior to World War I the principal advance in photogrammetry in the Western Hemisphere was pioneered by DeVille in Canada, the U. S. Geological Survey and U. S. Coast and Geodetic Survey in the United States with emphasis on terrestrial photography. In 1911 the National Research Council sponsored the development of a multiple lens camera. J. W. Bagley and F. H. Moffet of the Geological Survey perfected plans

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[1] The Section of Anthropology met May 24, 1943. Doctor Ronald Lippitt delivered a paper entitled, "The Training of Participant Observers." No abstract was received.

[2] Manuscript of paper presented was approved for publication by Public Relations Bureau, War Department, on April 29, 1943.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 5, No. 8, June 1943.

This publication is distributed to Members and is published monthly from November to June, inclusive, at 109 West Chestnut Street, Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Executive Secretary: Eunice Thomas Minor.

Assistant Editor: Ethel J. Timonier.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

for this camera and in 1918 completed actual construction of a camera and transforming printer.

From 1920 to 1940 tremendous strides were made by government agencies and commercial operators in the development of methods and equipment. Foreign equipment was modified and improved, adapted to quantity production of large scale topographic maps. During this period the emphasis was unfortunately on large scale, detailed maps and the national mapping program was almost completely lost sight of.

In the present war, the high speed of mechanized units and the great areas covered in actual operations make small scale tactical maps more important. Our total mapping efforts still fail to meet all military requirements. Equipment and trained personnel are still engaged in mapping areas within the continental United States while hundreds of thousands of square miles in actual or potential combat areas remain unmapped.

Despite the tendency to ignore the use of photogrammetry for reconnaissance mapping, some development has been accomplished. The RCAF, in 1921, inaugurated a three-oblique camera program, plotting with the aid of grids. This method gives reasonable accuracy at reconnaissance scales in regions of moderate relief. The Survey of India has used high obliques extensively and in this country O. M. Miller has, with the single eyepiece plotter, mapped large areas in northern Labrador. In 1932, the photoalidade was designed by R. M. Wilson. This simple and comparatively inexpensive instrument has produced satisfactory reconnaissance maps.

Months before Pearl Harbor, the Army Air Force attempted development of a method which would insure rapid compilation of small scale charts over large unmapped areas. In February 1941, the Geological Survey undertook to aid the Air Forces in the development of a suitable method. The Alaskan Branch of the Survey had worked for years on reconnaissance mapping methods and at the time was using the Wilson photoalidade. Besides the personnel of the Alaskan Branch, many others in the Survey participated in developing the tri-metrogon method, under the sponsorship of Col. Minton W. Kaye, American Air Force.

The tri-metrogon system had only one objective, photogrammetrical compilation of aeronautical charts with all possible speed. Specifications called for photography at 20,000 ft., the flights separated about 20 miles. Military aviation accomplished this photography; the work of compiling agencies began with delivery of the prints. With a production line for charts as the goal, technique must be simple, and equipment portable and procurable under priority requirements.

The unique feature of the tri-metrogon method is horizon to horizon coverage, by means of three K-17 cameras fitted with wide angle lenses. The compilation units perform a number of operations in sequence, and the personnel is trained to perform single operations in groups. The operations include indexing, identification of geodetic control and picture points, tilt analysis, radial plotting and graphic delineation of horizontal directions to picture points. Existing geodetic control is used wherever possible, and astronomic positions are obtained with the prismatic astrolabe in other cases.

Succeeding steps include combining the radial plot of vertical pictures with paper templates made from the obliques, and the construction and joining of the slotted metal templates from many pictures to form a network of control for planimetry, which is delineated on the photographs by using stereoscopes. The planimetry is transferred from picture to manuscript with two new instruments, vertical and oblique sketchmasters. These are adaptations of the camera lucida for which credit is due Mr. J. L. Buckmaster.

A final proofreading against the photographs precedes photographic reduction to compilation scale and the inking preparatory to reproduction. Contours are added where necessary by use of the photoalidade.

The application of high obliques to the study of geological structure and geography are strikingly apparent on many of the pictures taken for charting purposes. Not the least of these applications will be the correlation of information on pictures. The results of glacial and tectonic actions as well as structural control of drainage appear at first glance.

Accuracy has proved to be adequate for pilotage use on a scale of 1:1,000,000 and detail more than adequate for 1:500,000. The average cost of planimetric compilation with tri-metrogon photographs has ranged from 5 to 50 cents per square mile. The addition of 500 or 1000 feet form lines averages approximately 20 cents per square mile in addition. These figures include total outlays for research and equipment to date but exclude the cost of obtaining photography and control.

Present military operations will undoubtedly result in many startling developments in methods and equipment.

SECTION OF BIOLOGY

May 10, 1943

Doctor A. E. Mirsky and Professor A. W. Pollister, Hospital of the Rockefeller Institute, New York, N. Y., and Department of Zoology, Columbia University, New York, N. Y.: *Studies on the Chemistry of Chromatin.* (This lecture was illustrated by lantern slides.)

Fifty years ago the chemistry of chromatin was an active field of investigation. It is a curious paradox that as the biologist's interest in chromatin grew, the biochemist paid less and less attention to this material (see Mirsky, 1943, for literature). With the development of genetics and especially of cytogenetics more and more remarkable properties were attributed to chromatin, and yet at the same time chemical investigation of chromatin languished. The great work of Miescher and Kossel on the chemistry of chromatin had considerable influence on protein chemistry and also on the chemistry of nucleic acids, but investigators in both of these fields were not greatly concerned, as Miescher perhaps even more than Kossel had been, with chromatin itself. In the long run, however, there could be little doubt that the theory of the gene would bring a revival of interest in the chemistry of chromatin, and signs of this revival have now been apparent for some time. There is, for example, Caspersson's work on the ultraviolet spectrophotometry of chromatin and there are the investigations of Stadler and of Hollaender on the wave-length dependence of the mutations produced by ultraviolet light. Investigations along these lines emphasize the need for a more direct study of the chemistry of chromatin. In this paper we present a brief summary of some of our work in this field.[1]

Miescher and Kossel used strong acids and bases to extract nucleic acid and the basic proteins (histones and protamines). Although it was with these preparations that the foundations of our knowledge of chromatin were laid, it is apparent that all of the substances prepared by Miescher and Kossel are breakdown products of the materials that exist in the nucleus. Obviously much less drastic methods of extraction are needed. We have found that neutral salt solutions suffice to extract nucleoproteins from the nuclei of many animal and plant cells. This method of extraction has a two-fold advantage over the procedure of Miescher and Kossel: (1) The extracted materials are less altered by the

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[1] The cytological part of these investigations has been aided by a grant from The Rockefeller Foundation.

reagents used; (2) It is possible to extract nuclear constituents from a far wider range of animal and plant cells.

(1) In the older procedure the basic protein was split off and extracted with hydrochloric or sulfuric acid, and the nucleic acid was subsequently extracted with alkali. With salt solutions the whole nucleoprotein complex is extracted. In Miescher's classical work on salmon sperm it was always inferred that protamine and nucleic acid exist in combination within the sperm cell, but a nucleoprotamine was never extracted as such. By means of salt solutions it is now possible to extract a nucleoprotamine complex from sperm. Strong acid and base, used by earlier workers, not only separated nucleic acid and protein from each other but also undoubtedly degraded the separated components. The nucleoproteins extracted by salt solutions, by contrast, are evidently highly polymerized; for they form strikingly fibrous precipitates, and when dissolved, are exceedingly viscous and exhibit birefringence of flow. Under the action of strong acid and alkali these properties are lost, as the large complexes are depolymerized.

(2) Most of the work of Miescher and Kossel was on fish sperm. A few other kinds of cells were used, but Kossel believed that histones are present in certain special cells only. An entirely different conclusion has been reached by extracting nucleohistones with salt solutions. Nucleohistones have been prepared from so many different animal cells, from plant cells, and even from bacterial cells, that it seems likely that they are universal nuclear constituents. It therefore becomes possible to consider nucleohistones to constitute a part of the germinal material, which the general applicability of the principles of genetics indicates must be essentially the same substance in all cells.

In referring to the older preparations of nuclear materials mention should be made of the works of Lilienfeld, Huiskamp, and Bang as well as that of Miescher and Kossel. By extraction with water the former investigators were able to obtain a nucleohistone from the thymus gland. This method has an exceedingly narrow range of applicability, being limited mainly to the thymus; but by means of it highly polymerized nucleic acid was prepared many years ago. We find that the nucleohistone prepared by extraction with water differs from that extracted by salt solutions. In absence of salt, or at very low concentrations of salt, some depolymerization occurs, and this is not reversed by dissolving the material in concentrated salt solutions.

The numerous nucleohistones (and nucleoprotamines) that we have prepared resemble each other in that they all contain desoxyribose nucleic acids and basic proteins. A question that immediately arises concerns the extent to which the desoxyribose nucleic acids and basic proteins prepared from different sources resemble each other. Concerning the nucleic acids it is important to know whether their content of purine and pyrimidine bases varies. According to the tetranucleotide theory of Levene a nucleic acid contains equal quantities of four different nitrogenous bases; but it should be noted that the methods of estimating the different bases are inexact and that such analyses as have been made on desoxyribose nucleic acids were on materials derived from only a few different sources. It would be unwarranted to suppose that the composition of all desoxyribose nucleic acids is the same. And yet a definite restriction can be placed on possible variations among all the nucleic acids which we have examined. A careful study of the Dische diphenylamine reaction shows that it can be used to measure the ratio of purine to pyrimidine nucleosides in a desoxyribose nucleic acid. We find that in every preparation we have examined the ratio of purine to pyrimidine nucleosides is 1:1; they all conform to the tetranucleotide theory.

Histones prepared from different sources resemble each other, as Kossel was the first to observe, in being precipitated by addition of ammonium hydroxide and in failing to dissolve in an excess of this reagent. This is an indication that histones have basic isoelectric points. We find that another point of resemblance among histones can be detected in their ultraviolet absorption spectra. Almost every protein has an absorption band in the ultraviolet with a maximum at 2800 A. U. In collaboration with Dr. George Lavin we have determined the ultraviolet absorption spectra of a number of histones from the nucleohistone complexes that have been isolated by salt extraction. Every histone so far examined has a band in the same region as other proteins, but the maximum is slightly displaced, being at 2750 A. U.[1] A further uniformity, of a negative nature, has been discovered in the composition of the different histones. We find that in no histone that we have isolated is there more than a trace of tryptophane. Since most proteins

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Caspersson has investigated the ultraviolet absorption spectrum of thymus histone and believes that the maximum of absorption lies at approximately 2850 A. U., and on this basis, in an extensive series of investigations, he has drawn far-reaching conclusions concerning the protein content of chromatin in many different cells. The position of maximum absorption which he has assigned to histone is far different from that which we find; and if our observations are correct, Caspersson's conclusions become untenable. In comparing our observations with Caspersson's it should be noted that his histone preparations contained so much nucleic acid that the absorption spectrum of histone could not be perceived directly from the optical observations, and it was accordingly necessary to subtract the absorption due to nucleic acid from the over-all absorption to determine the absorption due to histone. Our histone preparations are sufficiently pure so that no such calculations are needed; the position of the absorption band of histone follows directly from the optical observations.

contain this amino acid, its absence in histones is of value in defining this group of proteins, and it also serves as a criterion of purity in their preparation.

The source of these nucleoproteins is the cell nucleus. This is clearly indicated, first, by the chemical analysis (see Mirsky, 1943). Desoxyribose nucleic acid is a component of all cell nuclei, as shown by positive Feulgen nucleal reaction. (When there is any nucleic acid in the cytoplasm—for example, in the pancreas cell—the sugar is always ribose.) Furthermore, protamine and histone are types of protein known only from cell nuclei.

Cytological studies confirm the chemical analysis; for they show conclusively that the nucleoprotein isolated by strong saline extraction is derived from the cell nuclei (Mirsky and Pollister, 1942, 1943). Direct observations of the nucleus (liver cells and spermatozoa) show that it swells and goes into solution upon treatment with strong sodium chloride solutions. Furthermore, as one would expect, the yield of nucleoprotein from tissues is directly proportional to nuclear volume, inversely proportional to the amount of cytoplasm. The highest yields are from suspensions of spermatozoa; thymus, kidney, and liver give successively lower yields. Finally, from carp eggs (a huge cell with but a single nucleus) the yield is not great enough for a visible precipitate.[1] Using a modification of Stoneberg's citric acid method (Stoneberg, 1939) we have isolated nuclei from mammalian thymus and liver. The yields of nucleoprotein from these nuclei are very high, comparable with those from sperm suspensions in being nearly 90 per cent of the dry weight.

Thus chemical analysis and cytological observations agree in showing that the cell nucleus is the source of the desoxyribose nucleoproteins. Furthermore, the source must be the nuclear chromatin; for chromatin only is Feulgen positive; and the sperm nucleus, one of the best sources of nucleoprotein, is nearly solid chromatin, lacking the other usual nuclear constituents, the nucleolus and the nonstaining fluid (nuclear sap) between the chromatin strands.

There can be no doubt that chromatin contains nucleohistone (or nucleoprotamine). The significance which we attribute to this fact will depend to a considerable extent on how much of these nucleoproteins are found in chromatin. Miescher concluded from his work

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Professor R. R. Bensley tells us that from the nonnucleated red blood-corpuscles of mammals he has found it impossible to obtain any fibrous nucleoprotein by salt extraction. It is most interesting that, by contrast, we find that the nucleated red blood-cells of fishes and birds are among the most excellent sources of fibrous desoxyribose nucleoprotein.

on salmon sperm that approximately 90 per cent of the sperm head consists of protamine and nucleic acid. This conclusion was arrived at by determining the phosphorus and nitrogen contents of the sperm head and then analyzing isolated samples of nucleic acid and protamine for phosphorus and nitrogen. By comparing these figures it was possible to calculate what part of the nitrogen and phosphorus of the sperm head could be constituents of nucleic acid and protamine. Given his analytical results, Miescher's conclusion is plausible, though it would be far more convincing if it had been possible actually to isolate 90 per cent of the sperm head as nucleoprotamine. All of the nucleoprotamine present in the sperm head can be extracted by means of neutral salt solutions, and it can, indeed, be demonstrated that approximately 90 per cent of the sperm head does consist of nucleoprotamine.

Fish sperm presents certain advantages for the study of chromatin: a major part of this cell consists of chromatin, and large quantities of sperm are often available. A still more favorable material would be chromatin itself, isolated from other constituents of the cell. We have found that it is possible to isolate chromatin threads from a number of different cells. The method is simple; and large quantities of material can be prepared without great difficulty.

Our first success in preparation of chromatin threads came while we were working with a material from which the threads can be isolated with such ease that we had the good fortune to obtain them entirely by chance. If the erythrocytes of the salmon or carp (the latter is the most readily available large live fish in New York) are thoroughly washed with saline, much of their hemoglobin leaks out. These cells, suspended in saline, are readily broken in a high-speed mixer (such as the Waring Blendor) and on centrifuging one obtains a slightly colored, clear supernatant and a colorless sediment. Under the microscope the latter can be seen to consist almost entirely of a mass of delicate threads.[1] These are chromatin threads; which give a strong positive Feulgen reaction, and stain with other typical nuclear dyes, e.g. acetocarmine. Chicken erythrocytes, a more generally available material than fish erythrocytes, are another satisfactory source from which to isolate chromatin threads. The great advantage of the nucleated erythrocyte is that practically all of its cytoplasm is soluble in saline, but by modifying the procedure somewhat we have also prepared these threads from liver and thymus.[2]

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[1] There is always a small amount of contaminating material that stains with acid dye (light green). This is nonnuclear material. Microscopic study of the material at various stages in the procedure shows the light green staining material to be of two sorts, fragments of red blood-cell membranes and of the cytoplasm of leucocytes.

Many of these threads may, actually, be the individual chromosomes of the resting nucleus. More significant, from the standpoint of analysis, is the fact that the threads are uniformly staining masses of chromatin with no indications of interchromatin fluid or of nucleoli, the presence of both of which complicates a quantitative analysis of entire, intact nuclei. The chromatin threads may, for analytical purposes, be regarded as masses of pure chromatin; and the only nonchromatin material in a preparation of these threads consists of the fragments of cell membranes and cytoplasm mentioned above. Since these fragments are not dissolved by strong saline, they form the residue after extraction.

When chromatin threads are extracted with 1.0 M.NaCl all of their substance passes into solution—a process that can be followed with the microscope. The material in solution is exceedingly viscous and precipitates in fibrous form when, by adding to six volumes of water, the salt concentration is reduced. This fibrous material is a nucleohistone. If the nucleohistone from a known quantity of chromatin threads is treated with alcohol and ether and then dried it is found to be equivalent to 93 per cent of the weight of the material (chromatin threads + cytoplasmic fragments) from which it was prepared. The same yield of nucleohistone is obtained from chromatin prepared from fish erythrocytes, chicken erythrocytes, or calf thymus. Our yield of 93 per cent is certainly a low estimate for the nucleoprotein content of chromatin, for the chromatin threads cannot be considered to be free, by any means, of other cell components. The cytoplasmic fragments, which do not dissolve in 1.0 M.NaCl, appear to be sufficient to account for the remaining 7 per cent of the dry weight—so that, in fact, the chromatin threads are nearly 100 per cent nucleoprotein. There can be no more than traces of other material.

Thus far we have referred to these fibrous nucleoproteins as nucleohistone (or nucleoprotamine) because there can be no doubt that they contain both nucleic acid and a basic protein like that which Kossel first prepared from nucleated erythrocytes, and which he called histone. It is logical to inquire next whether in addition to histone there is any other kind of protein in these nucleoproteins. To answer this involves some difficulty, because when histone is split from the complex by addition of hydrochloric acid much protein still remains attached to nucleic acid. Is this residual protein of the histone type? The same question arises concerning nucleoprotamine because from this material, too, only a part of the protein is liberated by addition of hydrochloric acid. In this latter case there is the following good evidence that the residual protein is of the protamine type. A significant characteristic of protamine is its lack of tyrosine. The Millon reaction is a sensitive and specific test for this amino acid, and when this test is applied to the material that remains after protamine is split by hydrochloric acid from a nucleoprotamine, no tryosine can be detected. The residual protein is therefore lacking in tyrosine and, to this extent, it may be considered to be of the protamine type. By analogy it may be supposed that the residual protein in a nucleohistone is of the histone type. The absence of tryptophane in histones makes it possible to provide more direct evidence. By means of the Hopkins-Cole reaction exceedingly small quantities of tryptophane can be detected, but not in the presence of nucleic acid. It is possible to separate nearly all the protein from a nucleohistone by shaking a solution of this material with a mixture of chloroform and octyl alcohol (a method originally due to Sevag). The separated protein is rendered insoluble by this procedure, but it can be examined for tryptophane, and no more than traces are detectable. After prolonged shaking only a very small amount of protein remains attached to the nucleic acid. It can therefore be said that insofar as absence of tryptophane is a characteristic of histones, practically all of the protein in a nucleohistone is of the histone type.

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[2] Chromatin threads have also been isolated, independently of us, by Claude and Potter, from lymphocytes. In a paper that will soon appear their method of isolation will be compared with ours.

We have considered the possibility that some nonhistone protein is loosely attached to chromatin and is removed when chromatin threads are prepared. To find this protein, if indeed there is any such, we have split off protein from chromatin while it is still within the nucleus by adding hydrochloric acid to isolated nuclei (of the thymus gland). Separated protein was found to contain less than 0.05 per cent of tryptophane, so that it may be concluded, again, that very little, if any, nonhistone protein is present.

A final goal in an analysis of chromatin is, of course, the determination of the chemical nature of those most important functional constituents of chromatin, the genes. Our analyses indicate that chromatin, with which we associate the special functions of gene reproduction and gene action, is largely, if not entirely, composed of a unique substance, nucleohistone. Is the gene, then, nucleohistone? It is difficult to avoid the conclusion that the specialized functions of chromatin are properties of its specialized component, nucleohistone—a line of reasoning analogous to one by which it has always been assumed that the function of muscular contractility is intimately dependent upon the presence, in abundance, of the unique muscle protein, myosin.

The bond between histone and nucleic acid in a nucleohistone is firm enough to cause them to migrate as one component in an electric field and to remain together in a film when spread at an interface. And yet there is evidence that desoxyribose nucleoproteins dissociate to some extent even without addition of strong acid, alkali or concentrated salt solutions (saturated sodium chloride). The dissociation of nucleoprotamine can be demonstrated by placing it in a cellophane tube and dialyzing against a salt solution sufficiently concentrated (1 molar) to keep the nucleoprotamine in solution. It is found that protamine slowly diffuses through the membrane, leaving the large particles of nucleic acid within the membrane. The tendency of nucleohistone to dissociate when dissolved in 1.0 M.NaCl cannot be demonstrated so simply because the molecules of histone are too large to pass through a cellophane membrane. That there is an equilibrium between histone and nucleic acid when nucleohistone is dissolved in 1.0 M.NaCl can be demonstrated by adding an excess of protamine to the solution. The protamine competes with histone for nucleic acid and by precipitating the nucleoprotamine formed, the displaced histone can be separated from the other components present. Protamine added in excess can also displace histone from fibers of nucleohistone precipitated in physiological saline, indicating that even under more physiological conditions nucleohistone tends to dissociate. Histone can also be displaced from nucleic acid when protamine is added to chromatin threads and even when it is added to intact, isolated, nuclei.

These observations on the displacement of histone by protamine provide us with a much needed method of preparing histone without the use of hydrochloric acid. But surely of far greater significance is the demonstration that in chromatin there is a tendency for nucleohistone to dissociate. From the massive, indiffusible particles of nucleohistone small quantities of histone (a protein of relatively small size) are apparently constantly dissociating. If this process occurs in the living cell, and we have good reason to believe it may, it could play an important part in the mechanism of gene action, for it would mean that a protein component of the chromatin is diffusing out to other parts of the cell.

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SECTION OF PSYCHOLOGY

MAY 17, 1943

MR. DAVID SHAKOW, Worcester State Hospital, Worcester, Massachusetts: *Some Aspects of the Problem of Unsuccessful Personality Screening.* (This report is based on material supplied by Dr. and Mrs. William Malamud.)

The results of a preliminary analysis of the social psychiatric data obtained on a group of 49 mentally disordered soldiers who have broken down in military service are reported. This group was taken from a larger one of 110 patients studied from various angles: physiological, endocrinological, psychological, clinical psychiatric, and social-dynamic. The patients were divided into six groups: four types of schizophrenia, psychoneurosis and psychopathic personality. The schizophrenic classification was not according to conventional subtypes but rather according to preinduction status.

The trends revealed are as follows:

1. From the point of view of screening, two of the schizophrenic groups and the psychopathic personality group appear to consist largely of persons who could be relatively easily screened out at induction were some social background data made available. The other three groups (two schizophrenic groups and the psychoneurosis group) could not so be screened out. In the latter case, if it seems desirable to keep persons who fall into these categories out of the armed forces (the question of the desirability of selective occupation and mental hygiene in the army should be considered, however) then more searching technics of a psychiatric, psychological and perhaps physiological nature will have to be developed.

2. From the point of view of the background characteristics, the first three groups give evidence of poorer adjustment in most spheres, i.e., there is a greater frequency of family neuropsychiatric abnormality, disturbed home conditions and unsocialized personality development.

3. In the military situation, whereas the conditions which seem to play a prominent role in causing breakdown in the psychoneurotic and the two less disturbed schizophrenic groups are either socially oriented or the result of concrete difficulties, in the other three groups these factors seem to be mainly the result of inadequacies of a deep personal nature.

SECTION OF PHYSICS AND CHEMISTRY

May 8, 1943

Conference on "*Magneto Chemistry.*"

The Section of Physics and Chemistry held a Conference on "Magneto Chemistry," as the third in the series for the Academic Year of 1942–1943. Doctor Leonor Michaelis, The Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"The Interpretation of the Temperature Variation of the Paramagnetic Susceptibility," by J. H. van Vleck, Harvard University.

"The Magnetic Properties of Hemoglobin and its Derivatives," by Charles D. Coryell, California Institute of Technology.

"The Magnetic State of Semiquinones and Their Polymers in the Crystalline State," by Leonor Michaelis.

"Magneto-optic Rotation Effects in Solutions," by Charles E. Waring, Samuel Steingiser, and Herbert Hyman, Brooklyn Polytechnic Institute.

NEW MEMBERS

ELECTED MAY 18, 1943

ACTIVE MEMBERS

Angrist, Alfred, M.D., Pathology, Associate Professor of Pathology, New York Medical College, New York, N. Y.

Anson, M. L., Ph.D., Biochemistry, Director of Biochemical Research, Continental Foods, Inc., Hoboken, N. J.

Barnes, Marion David, Ph.D., Physical Chemistry, Instructor in Chemistry, Columbia University, New York, N. Y.

Cowles, Philip B., Ph.D., Bacteriology and Immunology, Associate Professor of Immunology, Yale University School of Medicine, New Haven, Conn.

Eberhardt, Jacob M., Geology, Mineralogy, U. S. Office of Censorship, New York, N. Y.

Harvey, Ethel Browne, Ph.D., Biology, Independent Investigator, Biology, Princeton University, Princeton, N. J.

Hurdis, Everett C., Ph.D., Chemistry, Research Chemist, U. S. Rubber Company, Passaic, N. J.

Kamm, Oliver, Ph.D., Biochemistry, Scientific Director, Parke, Davis and Company, Detroit, Mich.

Lynn, John G. IVth, M.D., Neurology and Psychiatry, Associate Director, Head Injury Project, Neurological Institute, New York, N. Y.

Maglio, M. Martin, M.S., Chemistry, Assistant Professor of Chemistry, St. John's University, Brooklyn, N. Y.

Mahoney, John F., M.D., Venereal Diseases, Director, Venereal Disease Research Laboratory, U. S. Marine Hospital, Stapleton, Staten Island, N. Y.

Ochoa, Severo, M.D., Biochemistry, Research Associate in Medicine, New York University College of Medicine, New York, N. Y.

Roberts, Merritt E., Ph.D., H. Pertussis Antigens & Antibodies, Department Supervisor, Lederle Laboratories, Pearl River, N. Y.

Shapiro, Arthur, M.D., Medicine, Assistant Visiting Physician, Kings County and Rikers Island Hospital, Clinic Assistant Physician, Jewish Hospital, Brooklyn, N. Y.

Singher, H. O., Ph.D., Biochemistry, Enzyme Chemist, Memorial Hospital, New York, N. Y.

Wasserscheid, August A., Ph.G., Chemistry, Vice President in Charge of Sales, Mallincrodt Chemical Works of St. Louis, New York, N. Y.

ASSOCIATE MEMBERS

Braker, William, M.A., Organic Chemistry, Associate in Medicinal Chemistry, The Squibb Institute for Medical Research, New Brunswick, N. J.

Newell, John M., D.Sc., Immunochemistry, Senior Chemist, Massachusetts Antitoxin and Vaccine Lab., Boston, Mass.

TRANSACTIONS

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SERIES II

VOLUME 6

Assistant Editor
ETHEL J. TIMONIER

CONTENTS OF SERIES II, VOLUME 6

PAGE

Title page i

Contents iii

Experimental Studies on the Dynamics of Human Walking. By HERBERT ELFTMAN 1

Basic Sources of Human Motivation. By ANDRAS ANGYAL 5

Interindividual Behavior among Animals. By FRANK A. BEACH 14

New Members 19, 80, 144, 168, 200, 270, 345

The Mexican Volcano Parícutin. By PARKER D. TRASK 21

Biological Photomacrography and Photomicrography with Kodachrome Film. By JAY T. FOX 30

Emergent Concepts of Modern Psychiatry. By IVES HENDRICK 42

Studies in Radio and Film Propaganda. By PAUL F. LAZARSFELD and ROBERT K. MERTON 58

Phylogenetic Trees. By GLENN L. JEPSEN 81

Cultural Approach to Personality. By MARGARET MEAD 93

Report of the Annual Meeting. By EUNICE THOMAS MINER 102

How Do Tapeworms of Herbivorous Animals Complete Their Life Cycles? By HORACE W. STUNKARD. Address of the Retiring President 108

The Pre-Iroquoian Occupations of New York State. By WILLIAM A. RITCHIE. (Abstract) A. Cressy Morrison Prize Winner, 1943 122

The Distribution of the Salamanders of the Genus *Plethodon* in Eastern United States and Canada. By ARNOLD B. GROBMAN. (Abstract) A. Cressy Morrison Prize Winner, 1943 123

Ends as Well as Means in Life and Evolution. By EDWIN G. CONKLIN 125

Pidgin English and Cross-Cultural Communication. By GREGORY BATESON 137

Announcement, A. Cressy Morrison Prize Contest, 1944 142

Recent Studies of the Structure of the Folded Appalachians in Tennessee. By PHILIP B. KING 147

The Emergence of Personality. By LAWRENCE K. FRANK 149

Applied Anthropology in the Dutch East Indies. By RAYMOND KENNEDY 157

Conference on "Energy Relationships in Enzyme Reactions" 167

The Geology of Mount Desert Island (Acadia National Park). By GEORGE H. CHADWICK 171

Endocrine Factors in Water Diuresis and Water Intoxication. By ROBERT GAUNT 179

PAGE
Biological Aspects of Mountaineering. By BECKETT M. HOWORTH........... 188
The Social Identity of the Jews. By JOSEPH BRAM......................... 194
Modern Evidences for Differential Movement of Certain Points on the Earth's Surface. By HARLAN T. STETSON.............................. 201
Effects of Liver Extirpation and Transplantation on Blood-Cell Formation. By W. M. COPENHAVER.. 223
Approaches to the Intellectual Aspects of Personality. By EUGENIA HANFMANN 229
Art and Regression. By ERNST KRIS.. 236
A Comparison of Negro-White Relations in Brazil and in the United States. By E. FRANKLIN FRAZIER.... 251
Glacial Geology along Alaska Highway from Dawson Creek, British Columbia, to Whitehorse, Yukon Territory. By CHARLES S. DENNY.............. 273
The Vitamer Concept. By DEAN BURK, MARIE L. HESSELBACH, DORIS F. MACNEARY, and RICHARD J. WINZLER.. 275
Behavior and the Frontal Lobes. By WALTER FREEMAN and JAMES W. WATTS 284
The Role of Fictions in Cultural Organization. By JAMES W. WOODARD..... 311

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 6 NOVEMBER, 1943 No. 1

SECTION OF BIOLOGY[1]

OCTOBER 11, 1943

DOCTOR HERBERT ELFTMAN, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *Experimental Studies on the Dynamics of Human Walking.* (This lecture was illustrated by lantern slides and motion pictures.)

Walking is one of the more intricate human activities. The superficial ease with which it is accomplished is due to the success with which Nature, after persistent trial and error, has brought together dozens of bones and hundreds of muscles under the co-ordinating control of a nervous system which can make even the complicated seem simple.

The problems connected with human movement are numerous, ranging from the evolution of the skeletal framework to the measurement of energy expenditure. A proper solution of these problems depends upon accurate knowledge of the fundamental mechanism of movement. This could not be obtained until quantitative experimental methods were applied.

One of the paradoxes of walking is the fact that if a person walks a mile on the level, at a constant average velocity, he has done no work on the body in a physical sense, since he finishes with the same potential

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[1] The Section of Geology and Mineralogy met October 4, 1943. Doctor Parker D. Trask gave an address entitled, "The New Mexican Volcano Parícutin, and its Geologic Setting," illustrated by Kodachrome slides. An abstract will be published in the December number of the Transactions.

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and kinetic energy as he had when he started. But there is no doubt that his muscles have been working. An accurate measurement of the amount of energy used can be obtained by measuring metabolism by standard methods. A person weighing 110 pounds would use from 40 to 80 large calories in transporting himself over the one-mile distance, the actual amount depending on just how he walked. If he walked in the most economical fashion, it would take him half an hour to accomplish the mile, and the energy used up would be capable of keeping an 80 watt bulb shining all the time he was walking. If, instead, he could seat himself in a frictionless wheel-chair and apply this energy to pushing the chair, it would be sufficient to push him up an incline so steep that he would gain over 1000 feet in altitude for each horizontal mile.

Why must the body use so much energy to accomplish the simplest type of locomotion? The answer may be obtained by physical analysis of the mechanism by means of which the body moves. The body consists of a number of parts, each one of which may be considered to move as an essentially rigid unit. By means of instrumental recording and photography, the movements and the forces associated with them may be analyzed.

When this is done, it is found that the velocity of the body is by no means constant during walking. Each time a foot is placed upon the ground, the velocity decreases, to rise again as the foot prepares to leave. The kinetic energy of each leg is greatest during the forward swing. It must then decrease as the foot is about to be planted.

These changes in kinetic energy must be brought about either by the action of gravity or by the action of muscles, since friction and wind resistance are small at the speed of walking. The work done by muscles can be separated from that done by gravity only by detailed kinematic analysis, although limiting values may be obtained by calculating kinetic and potential energies.

The upward and downward movements of the trunk have been accused of being the cause of the muscular work done during walking. That this is not the case with steps of moderate length is proved by the fact that the upward and downward movements of the trunk are of such a nature that gravity is responsible for the greater part of the fluctuation in kinetic energy of the upper part of the body. As the trunk rises in the middle of the step, its velocity decreases, the process being essentially one of the trunk vaulting forward, using the leg as a pole. The decreasing kinetic energy of the trunk is converted into potential energy.

The movement of the legs is a different matter. It is in accomplishing the leg movements that most of the muscular work is done. At ordinary speeds of walking, the forward swing of the leg must be initiated by the shortening of appropriate muscles. At the termination of the swing, the opposite set of muscles must go into action. By stretching these muscles, the leg slows down sufficiently to allow the foot to be planted on the ground.

The regulating factors for the pattern of movement and the energy levels of the body are consequently the muscles. They are suited to this purpose by their fundamental properties. When a muscle is activated by a nerve impulse, it becomes tense, pulling at both of its attachments. This process, in itself, uses up chemical energy. By shortening, the muscle can then do work on the body, as in increasing kinetic energy. If the active muscle is stretched, it takes energy away from the physical mechanism. When the active muscle neither shortens nor lengthens, it stabilizes the joint over which it acts.

When we walk on the level with an even rhythm, the muscles regulate the physical energy of the body in such a fashion that the amount of energy they take out of the walking mechanism during certain portions of the step is exactly equal to the amount of energy which they put in during the other portions. If the muscles were only able to save the energy which they take out and then give this energy back a moment later, walking would require much less effort. This seemingly simple arrangement is, however, one of which muscle is incapable. The energy which the muscle takes out is not only wasted as heat, but the muscle actually burns up chemical energy in the process. It is conceivable that artificial limbs could be designed in which this storage of energy could actually be accomplished.

More practical methods are available for decreasing the muscular energy used in locomotion. By choosing a moderate length of step and walking at the rate of about one and a half steps per second, the energy used is a minimum. Under these conditions, the leg swings with approximately the period it would have when suspended as a pendulum and consequently the greatest possible use is made of gravity.

Another method of reducing energy expenditure is to correlate the swinging of the two legs in such a fashion that one leg is gaining energy at the same rate as the other leg is losing it. This cannot be accomplished in walking but is approached in running. It is responsible for the fact that running is more economical than we would otherwise expect it to be.

Other factors, such as the change in efficiency of muscular contraction with the rate of contraction, and the utilization of the long muscles which span more than one joint, are significant in a more detailed consideration of the problem of locomotor energy expenditure. Even these factors are dependent on the mechanical structure of the body in their application. Properly co-ordinated management of the movements of the parts of the body furnishes a fundamental key to efficiency of movement.

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SECTION OF PSYCHOLOGY

OCTOBER 18, 1943

DOCTOR ANDRAS ANGYAL, Resident Director of Research, Worcester State Hospital, Worcester, Massachusetts: *Basic Sources of Human Motivation.*[1]

The holistic point of view in the study of human nature is now rather widely accepted. At present it is hardly necessary to enumerate the arguments for the need of studying the human being as an integrated whole rather than an aggregation of artificially created segmental functions. The timely task is not to justify the legitimacy and necessity of the holistic approach but to exploit fruitfully its implications in further theoretical formulations and in the actual investigation of the various phases of human activity.

Tonight, I wish to present some holistically oriented theoretical formulations with regard to the basic sources of human motivation.

Once the statement is made that "the person is a whole," a *gestalt*, or whatever term one may use to express the idea of an integrated totality, the need for further specification and elaboration arises. One wants to know what the specific features of this totality are that distinguish it from other totalities. One wants to know what the specific characteristics and laws are that apply to it.

The first specification one can make is that the person, or any living being, is a *dynamic whole.* There are objects more or less static in nature, such as geometrical forms, crystals, etc., that can be studied adequately as morphological configurations. Another class of phenomena can be adequately studied only dynamically. The living organism belongs to this latter class. This idea has been aptly expressed by Jennings in his statement that "the organism is a process." Even the structural features of the organism, such as the morphological pattern of the heart, or the lungs, have their full meaning and significance only in relationship to the function that they are carrying out within the total organismic process. Since the word organism frequently has by convention a morphological connotation, it may be said that the study of living beings in general, and of the person specifically, should be a study of the organismic total process; in other words, a *study of the processes of living.* The current use of the concept of life is derived mainly from physiology. In the present context, however, I will use this concept in a much broader sense and refer by it to a physiologically, symbolically (i.e. psychologically) and socially integrated total process.

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[1] See also, **Angyal, Andras.** "The Foundations for the Science of Personality." The Commonwealth Fund. New York. 1941.

The next step in specifying the general holistic concept of man will consist in an attempt to detect the broad, over-all pattern of the organismic total process. Our problem is, then, to describe the general pattern according to which life process takes place.

Current definitions of living derive mainly from physiology and organic biology. So great has been the influence of the static point of view that, even in the definition of a process like living, certain rather static features have crept in. Most, if not all, definitions of life imply a notion of a kind of pendular oscillation about a point of equilibrium instead of a progression in some definite direction. One speaks of disturbances of equilibrium and re-establishment of equilibrium as a basic characteristic of living; that is, a limited range of oscillation around a static point. This type of hypothetical model has been formulated in various ways. Sometimes life is defined as a periodic oscillation between anabolic and catabolic processes, a periodic cumulation and dispersion of matter and energy. The definition of life as a process of self-preservation is also an essentially static conception of living, implying as its goal the maintenance of a *status quo* and disregards the progressive, forward surging trend which, to my mind, is the most characteristic feature of living. The concept of homeostasis, which is a useful concept, if applied to certain highly standardized part patterns of the total process of living, is fundamentally extremely static and, hence, inadequate for the description of the total pattern of the life process.

The formulation that I am going to develop conceives the over-all pattern of living as a dynamic trend of progression in a definite direction and not as an oscillatory motion about a point of equilibrium.

In order to develop this idea, we have to examine briefly another concept; namely, that of the *autonomy* of the organism. By autonomy is meant that the human being, or any other organism, does not behave passively, does not act entirely under the compulsion of the impacts of environmental influences, but is endowed with a certain degree of self-government. The organism is not an inactive point in which various environmental causal chains intersect—as mechanistic theories want us to believe—but, to a certain degree, is the cause and active originator of its functions. We may say with Adolf Meyer that the organism is endowed with spontaneity. Or we may say that the organism possesses a certain degree of freedom, if we use this concept in the sense of Spinoza and call "free" an entity which acts according to its intrinsic law and not under the compulsion of exogenous forces.

This autonomy, this internal steering or self-government, is implied in all characteristic life-activities, such as selection, choice, regeneration, adaptation, self-regulation, etc. Only in an organism that is capable of self-directed activity are such functions possible. Even the phenomenon of tropism—which often has been used as a choice example for the demonstration of an assumed mechanical character of the life-processes—definitely involves autonomy of the organism. For instance, in the case of geotropism the plant has not a single but a variety of responses to the stimulus of gravitation. It may resemble a mechanical happening when the plant, in response to gravitation, sends its roots downward. However, in response to the same stimulus of gravitation the plant sends its stems upward and its branches sideward. It is easily demonstrated that the stimulus response process in general—though it has been frequently mechanistically interpreted—definitely implies autonomy, spontaneity of the organism. In psychology, a point of view is gaining increasing acceptance according to which the external stimulus is not a mechanical cause of the reaction of the organism but something that *prompts* the organism to respond *in its own specific way.*

The autonomy of the organism is restricted by outside influences that, with respect to the organism, are *heteronomous.* That is, the organism lives in an environment in which processes go on independently and without regard to the organism. Every organismic part process, and the life process as a whole, is the result of two components:—the organismic autonomy and environmental heteronomy. This relationship can be expressed by the ratio $a : h$. The values of a and h respectively vary greatly from occasion to occasion. In some of its manifestations, the organism is more self-governed, in others, more steered by environmental influences, but the value of a and h in this ratio never reaches zero.

Living beings belonging to different species vary greatly according to the weight that autonomous and heteronomous influences respectively play in their life. Thus, for instance, a plant is greatly dependent for its nutrition on the composition of the soil, while animals are able to search actively for proper food. The reproductive functions illustrate such differences particularly well. In plants, even the process of fecundation is largely heteronomous: it depends on such external factors as wind and insects. Animals, on the other hand, can actively seek a mate. These differences are not mentioned here in order to show that one form is more efficient than the other. The important fact for us is that in the life process of one species the external determination plays a greater and the internal determination a smaller role than in the corresponding life process of another species.

If one considers any individual life process as a whole, one discovers a definite trend in it toward an increase of the relative value of a in the $a : h$ ratio; that is to say, *a trend toward increased autonomy.* In other words, the organism—the human person being no exception—tends to proceed from a state of lesser toward a state of greater autonomy. With the exception of one important field which will be discussed later, all activities of the person seem to fall into this pattern, so that the trend toward increased autonomy appears as the most general, over-all pattern of the total process of living. The general dynamic trend of the organism seems to be directed toward the extension of its autonomous determination over an ever increasing field of events. This is a tendency to fit the biologically chaotic items of the environment into the pattern of living. It implies mastery and domination of the environment. The operation of some such fundamental tendency, as outlined above, could not escape the attention of students of human nature and it has found its way into practically all systems dealing with personality organization. In the psychoanalytic system, the concept of aggression (in the Adlerian system, the drive for power and for superiority) is considered as a fundamental urge. Aggression, combativeness, ascendency, self-assertion, or some equivalent urge or drive, or trait, is assumed by most students of personality. Ives Hendrick, the psychoanalyst, who will be the speaker at the November session of the Psychological Section of the Academy, felt compelled by the facts to introduce into psychoanalysis the notion of an "instinct of mastery," an idea which is closely related to the concept toward increased autonomy as described here.

Our description of the general pattern of the life process implies a definite view with regard to the organism-environment relationship. It implies that life does not take place within the organism, and it does not depend on intraorganismic relationships alone, but that it takes place *between* organism and environment. Organism and environment are considered not as separate entities but as two poles of a unitary process of living.

This statement may need some clarification. It is easy to demonstrate that it is impossible to draw a line of separation in space between organism and environment. It is impossible to tell just where the environment ends and the organism begins. For instance, does food become a part of the organism after it is ingested, or only when it reaches the lymph and blood circulation, or when it appears in the tissue fluids, or only when it is absorbed by the cells? The consideration of such questions could lead to endless hair splitting but not to any satisfactory solution. The obstacle is a fundamental one: it is an impossibility to describe a process in static terms. It is impossible to describe adequately organism and environment as *objects* and separate them in space. It is possible, however, at least theoretically, to state to what degree a given life process is internally determined; that is, self-governed, and to what extent it is governed by external heteronomous influences. For a dynamic consideration, environment and organism are not *objects* but *types of determination*. Dynamically, the organism is the autonomous, centralized determination and environment is the noncentralized, heteronomous determination within the total process of living.

Life is a unitary process having an environmental and an organismic pole. The realm or sphere in which the life process takes place may be termed biosphere. If one follows a holistic and at the same time dynamic approach, one does not study organic processes *and* environmental influences *and* their interrelationship, but one studies *biospheric occurrences*. Of course, one is free to make abstractions, if one only knows that he is making them, and to describe the biosphere with the subject as a point of reference or else to describe it with the object as a point of reference. In the first instance, the life process is described in terms of biological tendencies, drives, cravings, wishes and similar conceptions. Viewing life with the object pole as a point of reference, it appears as a constellation of *opportunities* and *contraventions;* that is, a constellation of such factors that either lend themselves to be constructively built into the pattern of the life process or else factors that threaten to hinder or break up the organismic total process.

Summing up the preceding discussion, we have described a tendency of living beings to move from a state of lesser autonomy toward states of greater autonomy, a tendency to master the environment. We consider this tendency as one *basic source of motivation.* Although this tendency manifests itself in the person in specifically human ways, it is a feature that the human being has in common with all living beings. In fact, if the description presented is correct, the tendency to move toward states of greater autonomy is the most general, over-all pattern in all forms of living.

Before discussing a second basic source of human motivation, a word about the psychological mode of functioning of the organism may be in place. The type of illustrations given thus far may give the wrong impression that in the preceding formulations only the type of phenomena that the physiologist deals with is being considered and the specifically psychological or mental functions neglected.

Our point of view on this problem can be stated very briefly. The characteristic feature of the mental or psychological functions of the person is the use of symbols. In fact, psychological activity is identical with symbolic activity. Time does not permit me to justify this statement. I may mention, however, that the equating of mental activity with symbolization is not a new idea, but has been for some time an important thesis in Adolf Meyer's psychobiology. The tremendously important role that symbolic activity plays in the total activity of the person lies in its extraordinary economizing value.

The ability to function symbolically, the ability to use substitutions, is a unique endowment that some living beings possess. The emergency of this new capacity, however, does not change the basic pattern of the organismic total activity. Through the ability for symbolization, the organism gains only a new method that opens up new ways to express and actualize its inherent basic tendency toward increased autonomy.

Thus far we have considered individual lives as if they were self-contained units. This, of course, is only an abstraction. The individual in general, and the person specifically, is not a closed and independent system. The human being is just one member of a phylogenetic chain, a member of a family, a member of a social group, a participant in a culture; in the broadest sense, a part of a cosmic order.

The integration of the person into the superindividual units of the social group, the assimilation of its culture, of its written and unwritten codes, are just as essential characteristics of personality as the more individualistic functions. The person is a larger unit than a mere individual organism because he includes those factors through which it functions, as a participant in the superindividual units of society and culture. Man, in his religious attitudes, experiences himself as a member of a meaningful cosmic order. Also, ethical and esthetic attitudes transcend the scope of a strictly individualistic existence and a definite trend toward superindividual goals in discernible in these regions.

The attitude of the person toward superindividual units is very different from the self-assertive tendency, from the trend toward increased autonomy. While the trend toward increased autonomy aims at the domination of the surroundings, the characteristic attitude toward superindividual wholes is rather a kind of submerging or subordination of one's individuality in the service of superindividual goals. In this later trend, a person seeks union with larger units and wishes to share and participate in something that he regards as being greater than his individual self. For this principle we propose the term "*trend toward homonomy*"; that is, a trend to be in harmony with the superindividual units of a social group, nature, God, ethical world order, or whatever the person's formulation of it may be.

It is in the realm of social life in which the trend toward homonomy most commonly finds outlet. In spite of all the power-seeking, competitive and plainly utilitarian aspects of living together socially, this is still the field in which people most commonly seek outlet for their need to belong, to participate, and to share. It is an essential characteristic of the human being that he is not complete as an individual self, and he experiences loneliness and isolation when he cannot share his experiences with others. Friendship and the several varieties of interhuman relations, which somewhat vaguely are referred to as love relationships, are expressions—clearly or vaguely—of the homonomous tendency. The experiences which go under the name of empathy and the phenomena of identification belong to this class.

The attitude of sharing and participating and the feeling of belonging are not restricted to interhuman relations, but involve, to some degree, the physical environment also. Corresponding to the two basic trends, we have a peculiar two-fold attitude toward the world around us. On the one hand, this world is, to us, a constellation of opportunities to be exploited and contraventions to be overcome or to be warded off. A world which is to be conquered, a world which we want to possess. On the other hand, this is also a world in which we want to repose, to be taken into, to belong. There are some experiences that bring this attitude sharply into focus. Such may be the feeling of the man who returns home after a long time, and experiences this feeling; "These are the hills and the meadows, and the house and the town where I belong." It is not necessary, however, to go for illustration to somewhat infrequent experiences of homonomy. In most instances, where we use the expression, "I like," whether it is to express an emotional attachment to another person, or to an object, or to a cause, a trend toward homonomy is recognizable. The specific feature of this trend is an experience of transcending one's strictly individualistic limits, a feeling of belonging to something larger than oneself.

The homonomous tendency is expressed, if not most frequently, but perhaps most clearly, in certain types of ethical, religious and aesthetic experiences, and in love relationships between people.

The range and direction of sharing and belonging may vary greatly. One person may turn toward religion, another toward some social unit; but everyone has to have some object for the expression of this homonomous tendencies. The objective existence of superindividual wholes is a problem for philosophy and the student of personality is not directly concerned with such problems. From his point of view, it is immaterial whether the person's formulations with regard to superindividual wholes are founded on reality, or are illusory. For the student of personality, the important fact is that a trend toward homonomy is easily discernible in everyone's life. Since this trend represents a powerful source of human motivation, it is a vitally important factor in personality organization irrespective of whether the superindividual wholes may be revealed by philosophical analysis to be real or fictitious.

The trend toward increased autonomy and the trend toward homonomy seem to form a dichotomy of diametrically opposed forces. In the first, one tends to master and govern the environmental happenings. One aims at achievement and conquest. In the second trend, the emphasis is displaced from the individual to the collective, to superindividual wholes, in which the person tends to participate and share.

The antagonism between these two principles is probably only an apparent one. They may be conceived as two phases of the same process:—in the trend toward increased autonomy the biologically chaotic items of the environment are fitted into the organization of the individual's life, while in the homonomous tendencies—which might be considered as a second phase—the individual seeks to fit himself into organizations larger than himself.

These two trends are regarded as representing the over-all pattern of the total life-activity of the person. It is believed that all phases of human life can be understood as specific manifestations, as part patterns of these two basic tendencies. The basic tendencies of the human being seek expression through numerous channels that are patterned by the autonomo-physiological structure of the human body, by social conditioning and by personal experiences.

The formulations that have been presented here are admittedly highly theoretical. The main justification of presenting these formulations is that I found them a helpful background—a frame of reference—in considering problems of personality, and it may be hoped that the schema may have some degree of general usefulness and validity.

SECTION OF ANTHROPOLOGY

October 25, 1943

Doctor Frank A. Beach, Chairman and Curator, Department of Animal Behavior, The American Museum of Natural History, New York, N. Y.: *Interindividual Behavior among Animals.* (Illustrated by motion pictures of the social behavior in alligators and rats.)

Comparison of representative types of interindividual relationships in animals at distinctive levels of the phyletic series suggest the existence of certain general principles of social relationships. Reliable evidence now available is too scant to permit the precise formulation of such principles.

Studies of unicellular organisms have revealed the importance of group reactions such as mass physiology responses of certain protozoa (increase in resistance to toxins), mating preferences in clones of paramecia, relationships between population density and percentage of survival, etc. Group behavior in insects is extremely variable and the social nature of various species within the group ranges from the more or less independent existence of the solitary wasps to the highly complex societies of some ants. It is to be noted that in many types of insects maintenance of the social order is essential to the perpetuation of the species. Some forms of orientation behavior possess significant implications in the field of social relationships.

Phenomena of mass physiology have been observed in connection with the locomotor and respiratory behavior of various fishes. Schooling reactions of certain species constitute another obvious example of interindividual control. Some types of fish, such as *Betta splendens* (Siamese fighting fish), are almost constantly aggressive. Adult males rarely tolerate the presence of another member of their own sex. In other species, a rudimentary type of social order is observed. Within a community of *Xiphophorus helleri* (sword-tail), males are almost always dominant over females. Sexually dimorphic characters are especially important in this connection. If the male's sword is removed his social status is lowered to that of a female. If a sword is artificially attached to a female she rises in the dominance order. Defense of territory is shown by many species of fish, and is especially strong in connection with protection of the nesting site. One other prominent type of individual interaction at the fish level is to be seen in the co-operative behavior between two members of a brooding pair. The responsibility of caring for the eggs and young is frequently shared by both parents.

Many amphibians and reptiles display aggregative behavior apparently in response to favorable environmental conditions existing in a given locality. Often such aggregations are associated with the elaboration of basic behavior patterns such as reproduction. There is some preliminary evidence to indicate that under laboratory conditions male alligators exhibit territorial defense reactions. In certain lizards (specifically *Anolis carolinensis*) mature males establish and defend geographical territories. Simulation of field conditions within the laboratory has permitted the observation that the male defends his territory against encroachment by any other male but welcomes the approach of any sexually receptive female.

Interindividual relationships among birds have been studied extensively. The most outstanding examples of this category of behavior may be gathered under the following main headings: migration, reproduction, social dominance, and territorial behavior. Little is known regarding the social relationships involved in migration reactions. It is believed that in many migrating species flock leadership changes frequently and is unrelated to dominance. Reproductive activities in some birds provide excellent illustrations of co-operation involved in nest building, brooding the eggs, feeding the young, and defending the home territory. Among those species that practice communal display and are highly gregarious, social stimulation of the breeding flock is of primary biological importance. In such instances, it has been suggested that there is a numerical threshold below which colony size cannot fall if reproduction is to occur.

The existence of social hierarchies in some birds is well known. Individuals within a flock of domestic hens tend to arrange themselves in a straight line pecking order with the result that the most dominant female may display aggression toward any other member of the flock without being attacked in return. The second ranking hen must suffer the attacks of the dominant bird but may in turn peck any other hen in the group without fear of retribution. Other individuals fall at successively lower points on the scale. Loosely associated with peck status are found various other social privileges. It has been pointed out that social status is not related exclusively to physical strength and size.

If several roosters are kept with a number of hens, the males tend to establish a social order among themselves. The least dominant rooster is not permitted to mate with the hens and may suffer "psychological castration" with the result that he loses all inclination to mate with the females. If, in the absence of any other male, he attempts to court the hens the latter frequently will not permit him to approach. Black-crowned night heron chicks show dominance in the nest. The individual whose beak is held the highest at feeding time receives the most nourishment. In adult herons the courtship activities involve establishment of social dominance of the male. This relationship is "symbolized" when the male's beak is held higher than that of the female. Until this dominance recognition is established, successful mating cannot occur.

Many species of birds appear to establish flock territories. In other species each individual maintains and defends its own home range. The male robin is a good representative of the latter type. Among the California quail each covey, varying from 8 to 25 individuals, inhabits a given range. Although the ranges of two covies may overlap somewhat, with the result that the two groups intermingle upon certain occasions, the covies do not remain united for any length of time, but separate into their original groups after a short delay. Experimental investigation of this behavior has revealed that a bird transported from one covey to another is quickly driven off. It is of interest to note that the stranger is attacked and expelled exclusively by members of his own sex.

Many mammals, such as prairie dogs and beavers, maintain a colonial type of organization. Although the physiological and psychological factors responsible for such behavior are not clearly understood, certain survival values are obvious (warning against predators, etc.). Other groups of mammals exist chiefly in herds or packs. The simplest form of such an organization consists of a large family group as has been reported to occur in the case of the wild horse, where one stallion dominates, protects and guides a brood of mares. In such instances, supernumary adult males are usually driven off. Among other ungulates, notably sheep, the older females usually dominate the flock under most conditions. Frequently, adolescent animals are segregated according to age groups. In domestic cattle, one old female usually leads the herd and is at the same time socially dominant in other activities.

Intra-group organization in mammals varies widely with the species. Among rodents there is no clear-cut example of a hard and fast social hierarchy. Experimental work with mice has indicated the importance of generalized bellicosity or aggressiveness which leads to an impersonal sort of group organization; but this result differs distinctly from a dominance-subordination system depending upon individual recognition (such as has been observed in domestic fowl). It is possible to create artificial societies of mice in which certain individuals are always dominant over others. Further, hereditary strains have been separated which react differentially to various social factors. For example, individuals from one strain may be completely suppressed by a single defeat, whereas members of another strain tend to become more aggressive as a result of unsuccessful combat. Investigations with mice have clearly shown the importance of previous experience of the individual in connection with the establishment and maintenance of social status.

Field observations on monkeys reveal various types of family or clan organizations. In the case of the spider monkey, adult males are usually the clan leaders. Large family groups inhabit fairly restricted home territories. In this species a rudimentary sort of co-operation is exhibited in response to real or threatened danger. The chimpanzee is recognized to be highly social in nature; and it is generally admitted that an isolated individual is not representative of the species. Individuals who have worked extensively with this primate have suggested that it inherits definite social needs or drives; and that in chimpanzee society the individual is constantly confronted with the necessity of maintaining its social status. Co-operative reactions are frequently observed. Two particularly significant aspects of interindividual relationships in the chimpanzee are to be seen in the execution of the grooming responses and in the dominance-subordination relationships between male and female. All other factors being equal, the male member of a pair is usually dominant, but during periods of sexual receptivity dominance is passed to the female. The dominance principle, as it operates in chimpanzee groups, appears to encourage initiative, responsibility and leadership; and to point to the establishment of a social system which places a premium upon social service and certain types of co-operation.

Experienced investigators working in the field of interindividual relationships have advanced the proposal that potential sociality is inherent in living organisms. This conclusion implies that "social life is not an accident appearing sporadically among a few highly evolved animals,—but rather a normal and basically widespread phenomenon" (Allee). The possible evolutionary importance of interindividual organizations is reflected in the belief of many students who suggest that a socially organized group may differ sufficiently from a non-organized one so markedly that natural selection can operate in favor of the more or less integrated population unit.

(Factual data and theoretical interpretations embodied in the foregoing discussion have been drawn from the work of many investigators. Since this presentation was intended as a more or less nontechnical summary of the field, direct reference to specific sources has been omitted. However, major indebtedness to the publications of the following scientists is gratefully acknowledged: W. C. Allee and his collaborators at the University of Chicago, R. M. Yerkes and the staff of the Yerkes Laboratories of Primate Biology at Orange Park, Florida, the late G. K. Noble of The American Museum of Natural History.)

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY*

DOCTOR PARKER D. TRASK, Geologist, U. S. Geological Survey, Washington, D. C.: *The Mexican Volcano Parícutin.*† (This lecture was illustrated by Kodachrome slides.)

The new volcano in Mexico, El Parícutin (pronounced Pah-*ree*-koo-teen), is a unique geological phenomenon; for, before our very eyes, it has sprung into existence and has grown to a very respectable height of 1500 feet, all within a period of 8 months. It lies within a region in which no previous volcanic activity has been known within the memory of man, though, in 1759, the volcano El Jorullo, some 50 miles to the southeast, likewise suddenly was born, grew to a height of more than 1000 feet within 5 months, and then quieted down, never more to erupt violently. Will Parícutin do likewise? That remains to be seen, for, at present, it is still going strong.

For the first time in their lives, geologists have been able to observe, in a single volcano, all stages of its history. Parícutin exhibits many of the features of other volcanoes; but other volcanoes have been encountered by geologists after they have been in existence for some time, and their early history is unknown. The early history of Parícutin, therefore, fills important gaps in our understanding of volcanism.

* The address entitled, "Variations in Geographical Co-ordinates in Relation to some Problems of Structural Geology," by Doctor Harlan T. Stetson, scheduled for the November 1st meeting of this Section, was postponed until a later date.

† Address presented before the Geological Section of The New York Academy of Sciences in New York, October 4, 1943. Published by permission of The Director, Geological Survey, U. S. Department of The Interior.

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To me, the most outstanding aspect of this volcano is the incredible rapidity with which it grew. Within one week it was 550 feet high, and within 10 weeks it was 1100 feet in altitude. Up to this time, all the material in its cone had come from fragments that had been blown into the air from the volcano. No lava came from the cone until nearly four months after the eruption started; and then, contrary to some popular reports, it did not flow over the lip of the crater. Instead, it broke through the sides of the cone, undermining the overlying fragmental material. Lava appeared within two days of the first explosion, but it issued quietly from a fissure about 1000 feet north of the explosive vent.

Geologists have been observing Parícutin practically from its inception. Dr. Ezéquiel Ordoñez, the grand old man of Mexican geology, despite some eight decades of age, reached the volcano, together with some associates from the Instituto Geológico de Mexico, within two days of its birth; and he has actively been watching its development ever since. Senor Téodor Flores, Director of the Instituto Geológico, has devoted all available facilities of his institution to the study of Parícutin, and the passionate interest he has shown in this volcano would gladden the heart of any scientist.

Dr. William F. Foshag, of the U. S. National Museum, in charge of the war-minerals work of the U. S. Geological Survey in Mexico, has been making a systematic study of Parícutin, and I am indebted to him for practically all statements in this paper not based on my own observations. In addition, many other geologists have visited the volcano. Therefore, eventually, a rather complete record of its history will be available. I saw Parícutin three times: first, a week after its birth; a second time, when it was nearly three months old; and once again, a month later, when I flew over it in an airplane, during one of the stages when lava was pouring from the cone.

Parícutin is located in the state of Michoacan, 200 miles in airline due west of Mexico City in the Sierra Madre Occidental, which forms the west boundary of the high plateau that occupies the central part of Mexico. The volcano is situated in an area of forested hills and cultivated lowlands, and the base of the cone lies about 7500 feet above sea level.

Parícutin is readily accessible. In the early stages an automobile could be driven to the front of the advancing lava flow, but now so much ash has fallen that the only practicable approach is by road to San Juan de Parangaricutiro, where horses are available for visitors to the volcano, four miles distant. San Juan is 15 miles by passable dirt road from Uruapan, which, in turn, is 300 miles by excellent paved road or by rail from Mexico City. Frequent comfortable bus service is maintained between Mexico City and Uruapan, and the ride is one of the most beautiful in the whole world. Good accommodations are to be had in Uruapan, and frequent bus service to San Juan is available. Costs are surprisingly low.

Parícutin is located in a region of volcanic rocks consisting of essentially the same andesitic basalt as its own lava. Several hundred volcanic cones lie within a radius of 75 miles of the volcano. These are of all ages; some are so fresh that they can hardly be more than a few hundred or a few thousand years old; others are so dissected by erosion that they must be many tens of thousands of years in age. Most of them are cinder cones—that is, cones composed of debris blown from a vent in the ground; others are composite cones, consisting of both lava and fragmental material. They range in height mainly from 200 to 800 feet. The highest rises some 4000 feet above the surrounding country. The soil is rich and is derived from volcanic ash and interbedded lava. Most of the cones are conical and have small craters, but a few consist of rings of fragmental material, 200 feet or less in height and some hundreds of feet in diameter. One such abortive cone is situated about one mile northwest of Parícutin; another lies some miles to the east. From the air, this latter cone seems to be some 3000 feet in diameter and less than 200 feet in height. It contains within it, but somewhat off center, a similar ring-like ridge about 1500 feet in diameter.

The first intimation that something was about to happen was an account in the newspapers, about February 12, 1943, that 25 to 30 earthquakes had been felt the previous day near the town of San Juan de Parangaricutiro. Each day, thereafter, increasingly more tremors were reported, and on February 19 some 300 earthquakes occurred. The next day the eruptions started.

Stories of the beginning of the volcano are legion, and, as time goes on, they probably will become more varied. One of the most colorful is that a farmer, while plowing his field, turned over a stone, whereupon lava gushed forth and, like the headless horseman in the Legend of Sleepy Hollow, raced down the furrows behind him as he fled. This story of course is fantastic. In the first place, no Mexican would plow a furrow down hill, and, in the second place, the Parícutin lava was too viscous to flow rapidly.

The most reliable story is that a farmer, Dionisio Pulido, while plowing, noticed a column of smoke, about three inches in diameter, spiraling upward from a small hole in the middle of the field. Thinking that he had inadvertently started a fire, he went over to the smoke and put it out by placing a stone over the hole. He continued plowing, and sometime later looked around and saw smoke emerging from the ground in greater force. He went, forthwith, to inform the Presidente of the town of San Juan, who sent a group to see what was happening. Upon arriving at the spot, three hours later, these people found a hole some 30 feet in depth from which dense clouds of dark smoke were issuing. About ten that night, February 20, the first explosion occurred, and since that time the volcano has been erupting steadily.

When I first visited the volcano, on February 28, a little over one week after its inception, the explosions were coming at fairly regular intervals of 4 seconds. At times, two explosions would come in quick succession; at other times, the interval between outbursts was 6 or 8 seconds. In general, the explosions were of about equal force, though occasional loud outbursts occurred. One was strong enough to knock me off balance while walking some 3000 feet from the crater.

The sound from the explosions seemed to originate within the crater at about the level of the ground, though occasional explosions took place in the ash cloud 500 feet above the top of the cone. Each explosion from the crater acted like a giant gun-burst. The material was ejected from the throat of the volcano in a cylindrical column to a height of 400 to 800 feet above the top of the volcano, and, at this point, like water in hydraulic jump, suddenly formed dark expanding cumulus clouds of ash that billowed upward to a height of 6000 to 8000 feet above the ground, where steam would begin to condense. With increasing altitude the ash cloud became progressively whiter with water vapor until some 10,000 to 12,000 feet above the ground, where it was nearly pure white. The column of vapor continued upward to about 15,000 feet and was carried eastward in a horizontal cloud bank from which columns or large puffs of vapor curled upward for another 2000 or 3000 feet, like ostrich plumes sewed tandem on an ermine scarf.

At this time, the material ejected from the crater was thrown upward at an angle deviating from the vertical by 10°, much as if it were coming from a sharply defined conduit. As a result of this inclined direction of outburst, more material fell on the west side of the cone than on the east, thus causing the top of the cone to be lopsided. In the course of four hours, the angle of ejection changed gradually back to vertical and two hours later was deviating 5 degrees to the east, thus causing the east side of the cone to build up faster than the west side.

The column of ash ascended nearly vertically, but was deflected slightly eastward by the wind. Trains of cinders, one-eighth to one-half inch in diameter, rained down from the ash clouds on the lee side of the cone. They were cool, light, and very porous, and they sounded like sleet as they fell on one's hat. Few cinders were falling more than two miles from the volcano. At this distance the ground was just barely covered by them.

Fine particles of ash were transported greater distances than cinders, and covered the country side in delicate films for as much as 15 miles away on the leeward side. The ash and cinders were 18 inches thick, 500 feet from the edge of the cone, which was the closest to the volcano my courage permitted me to approach.

At this point, the ground was pock-marked with pits three to five feet in diameter, where large fragments or bombs had buried themselves in the ground. The average distance between bomb-pits, at this point, was about 20 feet. During some 30 minutes while I was standing there, two bombs fell within 300 feet. One bomb more than four feet in diameter landed 25 feet away. For awhile, as it was coming down, it looked as though it might make a direct hit, and the problem was which way to run, but eventually it veered slightly and the next moment it came down with a large whoosh and whistle, and buried itself. The top was about one foot beneath the surface of the ground. A piece broken from it was hot enough to light cigarettes. Another bomb, two feet in diameter, landed 50 feet to the rear, breaking an oak limb eight inches in diameter, much as if it were a cleaver cutting a bone. This bomb buried itself three feet in the ground. It came down five feet from some girls, who immediately retired to a more discreet distance.

Most of the fragmental material ejected from the volcano in this stage of its history consisted of bombs, rather than of ash or cinders. With each explosion, the bombs were blown 2000 to 3000 feet into the air. Most of them landed on the cone. The greatest distance at which I found a bomb was 3500 feet from the center of the volcano. The bombs went so high that it took from 10 to 15 seconds for most of them to fall, after they had reached their greatest height. They were roughly spherical and ranged in size from a walnut to a large house. They ranged in diameter mainly from three to five feet. The largest I saw was a block 50 feet in diameter, which was blown 300 feet above the top of the crater, that is, 850 feet above the ground. Nearly all the bombs, when they landed, were so thoroughly solidified as not to change in shape, though many, when they struck the sides of the cone, broke into pieces. A few were, still liquid when they landed, splattered out like pancakes on the ground. Bombs of this type did not penetrate the earth for more than three inches. Others rotated slowly in the air, gradually thinning in the middle, and, before they fell, separated into two tear-shaped bodies. Some, after coming to rest on the cone, smoked for a considerable time, certainly for as long as 15 minutes. Most of the bombs consisted of highly vesicular basalt, but a very small proportion were composed of a light medium-grained granitic rock that looked like diorite. These granitic rocks were angular, and not vesicular. They evidently were blown from the conduit through which the lava was coming.

Most of the bombs landed upon the sides of the cone, ricocheting down the side until they came to rest. The sides of the cones were remarkably even and were at an angle of 33° with the horizontal. The volcano at this time was 550 feet high and 1700 feet in diameter at the base. The diameter of the crater at the top of the cone was 250 feet, and the orifice from which the material was ejected seemingly was about 75 feet in diameter.

The volcano, at night, is a magnificent, never-to-be-forgotten sight. Nearly all the bombs that are blown from the crater are red hot, and they shower up like a gigantic Fourth-of-July flower-pot. The floral effect is complicated by the fact that four or five subsequent explosions have taken place before the bombs from any one explosion have all landed. Thus, some bombs are going up, some are just arching over at their highest point, and others are falling. After the bombs strike the sides of the cone they cascade down in great fiery arcs. Some come to rest on the sides; others roll to the bottom. The glow from the cone comes and goes, depending upon the number of bombs that fall and the interval between explosions. Big outbursts cover the whole volcano, and the cone progressively lightens up in an ever larger descending curtain of fiery red, as the fragments land progressively down the sides of the cone. Then, as the bombs cool, the red gradually darkens. Yet, before the color finally vanishes, another crop of bombs falls and the scene is repeated. Even though parts of the cone may fade into darkness before a succeeding increment of glowing bombs descends, a ring of red always remains around the edge of the cone where the rocks that roll completely down the sides come to rest.

A flow of lava first appeared in a plowed field about 1000 feet north of the crater, about two days after the birth of the volcano. In five days it had attained a length of 2000 feet, a width of 600 feet, and a thickness of 20 feet at the sides and front. It continued to grow for about 6 weeks, until it was about 6000 feet long, 3000 feet wide, and more than 100 feet high. The front and sides were steeply inclined; the top was nearly flat and consisted of blocks of congealed, scraggly aa lava, 3 to 15 feet in diameter.

At the time I saw the lava, five days after it first appeared, it was flowing westward down a gently sloping field at a rate of about three feet an hour in front and one foot an hour on the sides. Like a glacier, the lava moved most rapidly in direction of greatest slope, and, like a glacier, it also developed pressure ridges as it flowed. It advanced by pushing large blocks of solidified lava, three to five feet in diameter, off the front and sides. These blocks fell down the edges of the flow, and, in turn, were covered by other blocks similarly spalled by the advancing lava. Gradually, the molten rock inside the flow passed over the fallen blocks and incorporated them within itself, forming a volcanic flow breccia. At all times, the surface of the lava was congealed, except for places from which blocks had broken off at the edges of the flow as the lava advanced. These freshly exposed places consisted of red, pasty, dense lava that solidified within a few minutes to hard rock.

Fumaroles came out of vents 6 inches to a foot in diameter and gave off dense clouds of white smoke, which, according to Foshag, is largely ammonium chloride. The ammonium chloride also condenses in a white powder around the orifices of the fumaroles, and, in places, a fringe of bright-orange iron chloride is also formed. Few poisonous gases seemingly are given off.

When I visited Parícutin the second time, about the middle of May, nearly three months after its birth, it was still erupting at about the same rate, but the explosions were less forceful, and a much larger proportion of ash was coming out. The cone had doubled in height and the orifice from which the material was being ejected seemed to be about 150 feet in diameter. Ash was everywhere, and most of the trees within 5 miles of the volcano had been killed. The lava flow had stopped moving and was covered by 6 to 8 feet of ash. Even at Uruapan, 15 miles east of the volcano, the ash was 6 to 8 inches thick, and at San Juan, four miles to the west, it was 15 inches thick. A large part of this ash fell in one period of 36 hours, early in April. Electrical discharges or lightning strokes were flashing at irregular intervals, sometimes as frequently as 30 an hour. These flashes were vertical in the cloud of ash, and generally started within a few hundred feet of the top of the cone. They ranged in length mainly from 500 to 1500 feet, and produced sharp cracks but no loud thunder.

About four weeks after this visit, a phase of lava activity ensued. In the course of a week, 8 flows appeared, all from within the cone. Prior to this time, that is, for almost four months, no lava had come from the cone itself and there had been just the one flow. According to Foshag, each flow was preceded by a period of violent explosive activity, which terminated shortly before the lava came. While the lava was issuing from the volcano, relatively few explosions took place. All these flows ruptured the sides of the cone, and those that came from the upper part of the volcano undermined the fragmental material above, leaving a large gap in the side of the cone. These gaps were rapidly filled by material blown from the crater after the lava ceased to move. One flow advanced in three days as far as the town of Parícutin, three miles to the west. When it approached Parícutin it was moving at a rate of about 100 feet an hour. Another flow on the east side of the cone spread out like a large fan at the base of the volcano. According to Foshag, this one went 1500 feet in 15 minutes.

While one of these flows was in progress, I had the good fortune to fly over the volcano. At this time, the crater was nearly full; lava extended to within 50 feet of the lip. The top of the lava in the crater was congealed, and was broken in large blocks. Ashes were issuing from a vent estimated to be 75 to 100 feet in diameter, in the northwest part of the lava field within the crater, but relatively few explosions were seen. Lava was flowing from an opening on the east side of the cone. This vent was 50 to 75 feet in width and 200 to 300 feet in height. The upper limit was 100 to 150 feet beneath the lip of the crater. The lava coming through the opening was red hot, but it soon cooled and congealed while flowing down the sides of the volcano. The cone was intact above the point of escape of lava, but, according to Foshag, it subsequently was undermined by the flow.

At this time, June 19, Parícutin was 1200 feet in altitude. By late

September, it had reached a height of 1500 feet. At that time, it was still exploding at about the same rate as when it started, and was showing no signs of dying. In the meantime, several other flows of lava had appeared, mostly from within the cone. Parícutin, truly, is now a full-fledged volcano.

SECTION OF BIOLOGY

November 8, 1943

Mr. Jay T. Fox, Fox Museum of Natural History, Seaford, L. I., New York: *Biological Photomacrography and Photomicrography with Kodachrome Film.* (This lecture was illustrated with Kodachrome slides.)

In this present age of enlightenment, there is no field of endeavor, achievement or research, which does not at some time or other call upon photography to illustrate or aid in its examination. The camera, popularly used for recreation, now with color film, supplies the most reliable means of recording facts and conditions in the work of a biologist. Biological nature, in its every mood, presents a pleasing arrangement of color, unassisted by man. Through the medium of color photography, a basic knowledge of life and the world in which we live is revealed in such a way that it is readily comprehended by every student of biology. Photography, for the past century, has been largely restricted to the black and white process which performs well with respect to line and shape, but renders colors only in the monochromatic tones. As pointed out by Stenger,[13] the history of color photography has developed concurrently with the closest relation to that of black and white photography. Simplified processes of color photography, of late, have been eagerly accepted by the majority of scientific photographers, because of the true tonal reproductions of the original. Color photography is unquestionably most valuable in this work, as it conveys vivid experiences of accurate mental concepts to the minds of those engaged in biological research. My experiments with other color processes have proved to me that Kodachrome K135A film, when used with the proper light source[5] for biological photography, produces the truest color rendition possible. Once the fundamental problems in color photography are understood, the successful use of Kodachrome film depends only upon care in lighting, proper color temperature of the illuminant, and correct exposure time. Before detailing the equipment and technique I use in my research, it might be well to explain this subtractive color process. The chemical three-color emulsion subtractive process goes back to the work of Dr. Rudolph Fischer in Berlin (1910–1914). The

Kodachrome method was invented in 1921 by two musicians, Leopold Mannes and Leopold Gadowsky, who used it first as a hobby and later perfected it in the Kodak Research Laboratories. However, it was not until 1936 that the Eastman Kodak Co., of Rochester, N. Y., introduced it commercially for natural color miniature photography.

Kodachrome film carries three emulsions on one face, separated by gelatin layers, each selectively sensitized to one of the three primary colors. The emulsion nearest the film base responds to red light, the middle emulsion to green, and that at the surface to blue. A yellow dye above the middle emulsion prevents the blue light from reaching the two lower emulsions, since these are also sensitive to blue, in addition to green and red, respectively. The layers, so thin that their total thickness scarcely exceeds that of the emulsion layer of a black and white film, are coated on a safety film base having an anti-halation backing.

After exposure, all three emulsions are first developed to negatives. Then the film is re-exposed and developed in "coupler developers," so that, in the final result, the negative silver images are accompanied by positive silver and dye images in each emulsion layer. A coupler developer differs from ordinary developers in that it not only converts silver bromide to metallic silver, but, at the same time, deposits a dye of a predetermined color along with the silver. Both silver images are then dissolved away, leaving only the composite dye image. The top layer contains an image in yellow dye, the middle one in magenta dye and the bottom one in a blue-green dye. These colors are complementary to the colors to which the emulsion layers were originally sensitive. The final image is so balanced in color that the Kodachrome transparency, when projected with a high efficiency tungsten lamp, resembles, chromatically, the original object as it was seen by the eye.

Intermediate colors and mixtures are secured by partial absorption at each layer. Heavy dye deposits in all three layers subtract light of all colors, resulting in a black screen image. The finished Kodachrome film is free from all grain, as the silver grains, after serving their purpose of recording the original photo image in the film, are removed in the final stage of this process. The final color images are made up of dyed gelatin, so no grain remains in the final transparency. Thus, in this process, sharp images of fine detail are assured.

Success in Kodachrome photography depends largely upon accuracy of exposure.[2] The chances of exposure errors in color work are much greater than in black and white, due to the very limited latitude of color film. Latitude can be briefly defined as the ratio of maximum to minimum useful exposure. In black and white film, as pointed out by Roebuck and Staehle,[11] this may be as great as 10,000 to 1. The longer the exposure, the darker the negative; hence, longer time is required in printing a positive. However, in Kodachrome film, no such latitude is available. The exposure time is extremely critical.[4] For best possible results, exposure should be correct to within half a stop. One lens diaphragm stop* more than correct exposure will result in a thin washed-out transparency, while one lens diaphragm stop less than correct exposure will result in heavy, blocked-out shadows. It must be remembered that the Kodachrome process is a reversal process, therefore, the greater the exposure, the lighter the finished transparency; consequently, the colors appear washed out. When under-exposed, the colors appear too dark, sometimes black.

Photomacrography differs from photomicrography in this respect: a photomacrograph is a magnified photograph of an object as seen with the unaided eye; whereas, a photomicrograph is a photograph of a microscopic object. For the former, lens extension tubes are inserted between the camera and the lens; for the latter, the camera minus the lens is attached to a microscope. In early literature of photography,[15] and in Germany and France today, the word microphotography is applied to the methods of obtaining enlarged images of very small objects, by attaching a camera to the microscope. About 50 years ago, in America and England, the word *photomicrography* was substituted in place of *microphotography* and the latter name restricted to methods of obtaining very small images, or minute photographs, of large objects or scenes, by reduction in an ordinary camera.

The photomacrographic[9, 10] and photomicrographic[8] units about to be described were designed by me especially for photographing small opaque specimens. With these units, incident light is employed as the illuminant.

CAMERA

Not only did I desire a camera for biological photomacrography and photomicrography, but one that was universal in its adaptations. I herewith state my reasons for judiciously selecting the 35mm. Kine Exakta, an ultra-precision built camera of wide range.

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* By the term "one stop" is meant the *2 times exposure difference* between any two adjacent settings of regular stop series: F/2, 2.8, 4, 5.6, 8, 11, 16.

1—The Kine Exakta can be used in all fields of nature and science, for not only laboratory photography, but also for normal outdoor photography.

2—The various lenses can be interchanged readily by means of bayonet-type fittings, and light metal extension tubes can be placed between the camera and the lens for photomacrography.

3—With a suitable attachment, this light-weight camera can serve atop a monocular microscope for photomicrography.

4—35mm. Kodachrome film is purchasable in this size at reasonable cost.

5—The *exclusive* feature is that the Kine Exakta is *the only 35mm. single lens reflex* precision camera available.

6—The troubles of parallex, or the use of a restricted image in the view finder, either or both of which must inevitably be found in the *twin lens reflex* camera when exposures are made at short distances, are entirely absent in this single lens reflex camera.

The range of work which this camera can undertake is practically limitless. The true color image on the ground-glass screen of this camera is precisely the same as that which will subsequently be recorded on the Kodachrome film. There is not a trace of difference between the two images, in focus, depth of focus, or field of the object included. In all other types of non-reflex cameras, some sort of an additional focusing device must be employed, whether it be a simple piece of ground glass or a sliding focusing attachment. These additional devices necessitate moving the camera before and after focusing, which I find is a serious detriment in precision work.

Focusing with this 35mm. camera is arranged just as in all other larger-sized true reflex cameras. An image is thrown from the camera lens, by means of an oblique mirror (beam splitter), upon a horizontal ground-glass screen. Due to the fact that the accuracy required in focusing the miniature negative is very great (for the circle of confusion in this size is reckoned as 1/750th of an inch), the ground-glass screen of this camera has been combined with a special focusing magnifier. These, together, resemble a single, rather thick plate of optical glass. The lower side is plain and very finely matted; the upper side is carefully adjusted, in such a way that the focal length of the lens thus formed is exactly on the plane of the lower side. Thus, when the eye is placed over the finder hood of this camera, a very bright and considerably enlarged image (4 x 6cm.) is seen, a highly advantageous feature in focusing.

A second magnifier is also provided, which again gives an image twice the normal size and shows the central portion of the image only. The accuracy of critical focusing is greatly improved by this additional magnifier and, at this point, is equivalent to focusing a picture 2¼ × 3¼ inches in size on the ground-glass screen, in which case the circle of confusion need only be reckoned as 1/200th of an inch. This simple focusing feature places the *single lens reflex* camera far above all others in the photomacrographic and photomicrographic work. This camera is equipped with a focal-plane shutter, to give a wide range of exposure values. The range of speeds is from 12 seconds to 1/1000th of a second.

WORK TABLE

Emphasis must be placed upon the importance of rigidity in performing photography of this nature. The slightest vibration or movement is sure to cause failure. For this type of work, I find that the work table should be mounted on a sturdily constructed support. The one I designed is mounted upon a massive cast-iron base. It is so arranged, that the work table top can be raised or lowered by means of an elevating gear or heavy adjusting screw, to give a variety of available heights for comfortable working position. It can be locked at any convenient height and made extremely rigid at that point. I use this table for both types of photography.

PHOTOMACROGRAPHIC ACCESSORIES

Camera Support.—The camera support that proved the most versatile was a table clamp which accommodated a vertical rod 1 inch in diameter and 10 inches in height. This is adjustable in height and can be rigidly locked in any pre-determined place by means of a winged nut at the table clamp. On top of this vertical rod, I firmly secured a 3-inch-diameter metal platform. A special precision geared mechanism is mounted upon this platform, giving, through microscopic movement, the necessary horizontal, lateral and angular adjustments required for critical photography. The camera is mounted in the usual way to the top of this geared arrangement. It is my theory that the movement of the entire camera with lens and accessories in place, through precision gearing, greatly outweighs the older method of attempting to accomplish precise focusing by the movement of only the lens itself through its helical focusing device.

Illumination.—Attached to the above camera support is a horizontal steel rod, 12 inches in length and ½ inch in diameter, that supports or carries the illuminating agent which is a single No. 1 floodlight. A ball-socket tripod head allows the floodlight and reflector to be tilted at any angle and locked securely in any position by means of the tripod head clamp. This horizontal rod also carries a synchronous motor-driven electric timer for second and split-second timing. It can be set for timing to a fraction of a second or to any number of seconds up to 59. This electric timer automatically controls the turning on and off of the floodlight, thereby assuring exact duplication of results. I have determined, by repeated experiment, that this method of controlling the actual exposure time is, by far, more exact than when you depend upon camera shutter timing. The floodlight is plugged into the side of the timer which in turn is connected to the 110-volt 60-cycle source of electrical supply. With Kodachrome K135A film, even the slightest difference in color[12] quality between photoflood lamps differing in efficiency is quite noticeable. I therefore recommend the use of a suitable color temperature meter to control the quality of illumination for obtaining proper color balance in the color transparency. If the color temperature of the floodlight is found different from that for which the film is balanced, it will be necessary to compensate for such difference by regulating the line voltage by either an adjustable rheostat or an adjustable auto-transformer. Another method of controlling color temperature (when no line voltage control is available) is by the use of a set of seven color compensating filters which are obtainable from the Eastman Kodak Company; four are of bluish tints and three of them are yellow, graduated in approximately equal color temperature steps, so that corrections possible with them are adequate for the light source employed.

Extension Tubes.—In photomacrographic work, light metal extension tubes must be placed between the lens and the 35mm. camera. They serve the same purpose as the extra long bellows do on the larger cameras, thus permitting sharp focus at distances nearer than the lens alone is capable of focusing. Every size extension tube, depending on its length, has its own exposure factor or F/value which is constant for every lens. For close-ups, when the subject is nearer than 8 times the focal length, it is quite important to allow for the change in effective

F/value, due to the increased distance between the lens and film. This change can be calculated according to the following formula:

$$\text{Effective F/value} = \frac{\text{Indicated F/value} \times \text{lens to film distance}}{\text{Focal length}}$$

The lens I prefer is an Exakta F/3.5 Anastigmat in a bayonet-type mounting. I find that when this Exakta lens (2 inch — 50mm.) is used with a combination of extension tubes, almost any size photograph can be obtained. The following tubes, when used with this 50 mm. lens, will give:

12mm. ext. tube	reduction of	4:1
30mm. “		1.5:1
42mm. “		1.2:1
60mm.	magnification	1:1.2
72mm.		1:1.5
90mm.		1:1.8
102mm.		1:2
150mm.		1:3
192mm.		1:3.85
240mm.		1:5
300mm.		1:6

Easel and Backgrounds.—I constructed a wooden easel and platform with a backboard 12 × 12 inches, having two 3 × 3 legs, upon which rests a removable specimen platform 3 × 12 inches. Both easel and platform are painted dull black. Backgrounds are partly a matter of individual judgment. Barkely,[3] in his macro work, uses colored cards, but I find that velvet produces a softer effect. I use an array of some twenty or more shades of velvet. I always select a background that is most complementary to the specimen being photographed. In some cases, to give the specimen the appearance of being suspended in mid-air, I support it on a piece of polished plate glass. The advantage of this arrangement is that shadows are entirely eliminated.

Technique.—When making Kodachrome photomacrographs, eliminate all camera vibration, or it will ruin what might otherwise be a good transparency. Furthermore, make sure that the optical components are absolutely clean. The following will explain the steps involved: I place the specimen upon the platform and select a velvet background. The single, No. 1 Floodlight (balanced in color temperature[7] of 3450 degrees Kelvin* for K135A Kodachrome film) is turned on to provide adequate illumination and properly light the specimen, then locked in place by means of the set screws. The camera is then brought into position, with the aid of the geared mechanism, until the image appears perfectly centered on the ground-glass screen. The platform holding the specimen is then manually moved backward or forward to bring the specimen into approximate focus. Next, place the second magnifier against the ground-glass screen, to get the sharpest definition possible: i.e., the individual facets of the compound eye of an insect. This hair-line sharpness is achieved through the use of the geared mechanism which supports the camera. The reflected image, in its full brilliance of color, continues to be visible on the ground-glass screen until the very instant of exposure. Extreme accuracy in focusing is an absolute prerequisite of success. A reading is then taken on a photo-electric exposure meter[12] and the proper exposure time calculated, taking into consideration the length of the extension tubes being used with the camera lens set at F/16 stop. Photo-electric meters are calibrated to read in foot candles. They give accurate readings of the amount of light reflected from the object. The dials are calibrated for use with black and white film and cover a contrast range of 1 to 128, whereas, the range of Kodachrome is 1 to 4. Great care must be emphasized and exact readings taken on the photo-electric meter. In Kodachrome, with a range of 1 to 4, exposure must be right and the density exact, or the color rendition will not be accurate.

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* Color temperature is the temperature, expressed in degrees Kelvin, to which a "black body" must be heated, in order to emit light of the color required. The Kelvin scale has its zero at -273 degrees Centigrade.

Exposure.—When I am ready to make the exposure, the floodlight is extinguished and the camera lens stopped down to F/16. The camera shutter is tripped to its open position by flexible cable, to prevent any jarring of the camera. The exposure is now made by the synchronous timer which automatically turns on and off the floodlight at the predetermined setting. The actual exposure time varies with the color of the specimen being photographed, the length of the extension tubes being used, the color of the background, etc. After the synchronous timer extinguishes the floodlight, the shutter can be tripped manually, closing same. The film is then moved to the next frame and I am ready for the next photomacrograph.

PHOTOMICROGRAPHIC ACCESSORIES

Camera and Microscope Support.—For photomicrographic work I use the same type camera support as previously described, but I replace the 10-inch vertical rod with one 18 inches in height. A special right-angled fitting is provided, the vertical portion of which moves up and down upon the vertical rod, and, by tightening a clamp, can be secured at any desired height. The horizontal portion of the aforementioned fitting accommodates a ½-inch diameter steel rod or cross arm, 12 inches long, which supports the microscope.

MICROSCOPE.—The only part of a microscope that I use in this branch of photography (opaque specimens—not slides)[1] is the observation tube (160 mm.) of a good research monocular microscope, with its rack and pinion and ball-bearing vernier attachments. The base, mirror, condenser, and substage are not used. This observation tube, with its rack and pinion, is mounted, through suitable fittings, to one end of the above-mentioned cross arm. I find that, by equipping the observation tube with a special photographic objective (Bausch and Lomb — 48 mm.) with iris diaphragm, and using this objective with either a 4x or 6x ocular lens designed especially for photomicrographic work, ideal magnification follows in the finished transparency.

ILLUMINATION.—The most important factor in photomicrographing gross specimens is critical illumination.[6] Diffraction caused by too much uncontrolled light will result in failure. *The light which passes through the objective is the only light used in this color work;* hence, only that portion of the gross specimen directly under the objective should be illuminated. I illuminate the opaque specimen with a small focusing 8-volt spotlight. A transformer with a ratio of 110 to 10 volts supplies the power to light it. An external variable resistor decreases the 110-volt input voltage to the transformer in graduating steps which, in turn, reduces the secondary voltage. This resistor must be adjusted each time the light is used. This adjustment is made prior to photographic exposures, by means of a color temperature reading *of the light that has passed through the optical system of the microscope* and the voltage adjusted as correctly as possible to give a 3450 degrees Kelvin reading. With proper focusing of this spotlight, any degree of intensity can be obtained on any area of the specimen being photomicrographed.

CAMERA ADAPTER.—A micro-adapter, especially designed for use with the above 35 mm. camera, is needed for connecting the camera to the microscope. In photomicrography, the camera lens serves no useful purpose; consequently, it is not used. The ocular lens of the microscope serves in its place. The large end of the adapter is attached to the front of the camera by means of a bayonet-type connection, and the small end is attached to the upper part of the observation tube of the microscope by means of a suitable clamp. The adapter is hinged, thus allowing the camera to be swung out of the way when it becomes necessary to change the ocular lens in the microscope. One, or all three extension tubes (10–20–60 mm.) are used to give the proper size picture on the ground-glass screen of the camera. These are inserted between the camera and the microscope.

SPECIMEN PLATFORM.—A 6-inch diameter, semicircular ball-socket mechanism, with a flat top and supporting ring into which it sets, is used to hold the specimen. Orientation of the specimen itself is made possible in all planes and directions by using this specimen platform. In this way, I am able to bring any area to the opaque specimen in parallel with the horizontal plane of the film in the camera.

TECHNIQUE.—Correct technique is of paramount importance in producing photomicrographs of high quality. The biological specimen to be photomicrographed is placed upon the specimen platform and critically orientated. The light is then adjusted, so that the ray falls only upon that part of the specimen that is to be photographed. After a color temperature meter reading is taken, *at the microscope ocular,* the line voltage is adjusted through the variable resistor, to give the spotlight a color temperature of 3450 degrees Kelvin. The microscope is then carefully focused until a clear image appears on the ground-glass screen of the camera. This sharp resolution is accomplished by the ball-bearing vernier on the rack and pinion mechanism of the microscope itself.

EXPOSURE.—When I am ready to make the exposure, the spotlight is extinguished, and the iris diaphragm of the objective is stopped down to its smallest aperture. Just as in photomacrography, the camera shutter is set as for a time exposure, and the actual exposure is made by means of the synchronous motor driven timer, referred to previously. The photo-electric exposure meter is used for calculating the proper exposure time, and this reading is also made *at the ocular of the microscope.* The proper exposure time is then calculated, taking into consideration the length and number of extension tubes being used and the color of the specimen being photomicrographed. After the synchronous timer extinguishes the spotlight, the shutter of the camera is closed, the film moved to the next frame, and I am ready for the next photograph.

CONCLUSION

When a color print is required for illustrative purposes, the Eastman Kodak Company has a printing service available. These prints from Kodachrome transparencies are called Minicolor prints and can be secured in several sizes: 2¼ × 3¼ inches, 5 × 7½ inches, and 8 × 11 inches.

Excellent black and white prints can be obtained from negatives made from Kodachrome transparencies. A negative must first be made from the Kodachrome positive, preferably using panchromatic film, and then a positive black and white print is made the same size, or enlarged in the usual way. The enormous enlargements that are obtainable from Kodachrome 35 mm. transparencies firmly establish the miniature camera in the field of Biological Color Photography.

APPROXIMATE COLOR TEMPERATURES OF VARIOUS LIGHT SOURCES[14]

Source	Color Temperature Degrees Kelvin
60-watt vacuum tungsten filament lamp	2509
100 " gas filled " " "	2865
500 " " " " " "	2960
1000 " " " " " "	2990
500 " projection lamp	3190
GE Mazda lamp, 3200 degrees Kelvin	3200
Mazda C. P. lamp	3380
No. 4 floodlight	3415
No. 1 "	3444
Photoflash lamp No. 21	3800
White-flame carbon arc	5000
Mean noon sunlight at Washington, D. C.	5400
High intensity sun arc	5500
Direct sunlight about noon in midsummer may rise to	5800
Sunlight plus light from clear sky about noon as high as	6500
Light from a totally overcast sky may be as high as	6800
Light from a hazy or smoky sky may range from	7500–8400
Light from a clear blue sky	12000–27000
Direct sunlight early or late in the day in winter may drop below	5000

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SECTION OF PSYCHOLOGY

November 18, 1943

Doctor Ives Hendrick, Boston, Massachusetts: *Emergent Concepts of Modern Psychiatry.*

INTRODUCTION

Dr. Andros Angyal, in his book and recent address at the Academy, presented a concept of the processes we call "Life." This concept is based upon a carefully selected major premise. He assumes that an organism, especially the human organism, and its environment, together, constitute a functional universe. To this ecological identity of organism and its milieu, he gives the name "biosphere."

This is really, I think, a concept opposing, though emerging from, an implied premise that underlies most of the medicine, psychology, and sociology of the past. It denies the validity of the assumption, whether implied or explicit, that life is an aggregate, a summation, of unitary processes; that, for example, health is only the sum of a number of functioning organs that constitute the body; or that society is only the sum of the activities of several groups of humans; or, for that matter, that a bacterium is only the sum of the physiochemical reactions of its molecules. Rather, Dr. Angyal's concept of the biosphere assumes that every living process, the movement of a blood cell, a breath, an idea, or a social institution—a war, for example,—affects every other interaction between the biosphere and all of its parts.

Dr. Angyal no more considers a social incident, a handshake, for example, as an event relevant only to the life of each of two separate men than he considers a man to be the totality of mind and body; or, the body to be the totality of several functioning organ systems. Every heart beat is a function, not only of the heart, which may or may not have distant effects upon the nervous system, the digestive system, the life of a man. Every heart beat is a function of the total man. It determines and is determined by the function of the total man. Life, therefore, according to this emergent concept of the biosocial sciences, is the total interrelation of every part of the biosphere with every other part, and with the whole.*

To me, the significance of Dr. Angyal's philosophy is not that he gives us something brand new to think about, nor that he creates, in the philosophical tradition, a "logic-tight" system for argumentation pro and con, but that he has succeeded in making highly articulate a profound, inevitable, only occasionally focused, trend of all sciences today: What each of us here tonight is aware of thinking, with varying degrees of clarity and vagueness; what is objectively apparent in every science's accelerating tendency to align itself with its next of kin; what is increasingly obvious in such phrases as "the total situation," "the total organism";— all these represent deep currents of our mental age. He has not so much put a new record on the scientific phonograph as produced a loud-speaker from which the jarring scratches of a somewhat overplayed scientific philosophy are no longer audible.

Such trends in the basic premises of human thought take generations to evolve, and, when assimilated, they are, except by a few philosophers, scarcely recognized as implicit assumptions of the creative work and thought of any day. I doubt that Dr. Angyal's special voçabulary will survive a generation. However, I am convinced that, twenty-five years hence, the content of his thought will be so taken for granted, that youthful scientists of that future day will not even realize the basic ideas were new and challenging and baffling tonight.

That Dr. Angyal's conception does represent prescient insight into an inevitable cultural trend is shown in several ways. Thus, several of the most important contemporary schools of psychological thought imply much of what he make explicit. His theory owes most to *Gestalt* psychology. But the basic *Gestalt* assumptions are extended to include irrational phenomena of the mind, physiology, and social events, as well as those learning processes studied by Köhler. Psychoanalysis also assumes the basic postulates of Angyal's theory. What he calls "interference of systems" is identical with Freud's demonstration of "conflict." The "trend to homonony" is a basic assumption similar, though not identical, with what Freud called "libido," and, later, the "life instincts."* As Angyal points out, this trend to homonony is the dynamic factor in the socialization process described by Freud. The "trend to autonomy" is very similar to a force that I, under the name "instinct to master," have indicated is the dynamic factor in the development of the capacity to integrate by effective work. And the same idea also underlies the *"Funktionslust"* described by Karl Buehler, the "work performance" idea of Veblen, the "will to power" which is the important contribution of Adler, and the "integrative principle" of Clyde and Florence Kluchholn in anthropology and sociology.

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* In the words of Dr. Nolan Lewis ("Research in Dementia Praecox"), "There are many facts showing that the principle of unity is functioning throughout nature, and that each part, when it is sufficiently well known, is in a perfect integrative relationship with the rest." This idea of the organism is extended by Dr. Angval to include the environment. "Life," he says, "does not take place *within* the organism, but *between* the organism and the environment."

Nothing could show more clearly our indebtedness to Dr. Angyal, for this explicit and thoroughly thought-out presentation of these new hypotheses, than the present utter inadequacy of the psychiatrist's technical vocabulary. For, actually, our comprehension of many aspects of personality and the pathways of its development exceeds our capacity to express it simply. Even such a recent word as "psychosomatic" takes a paragraph to explain, to those who have not yet found it essential for their daily work. And, such a classic of the psychiatric lexicon as "schizophrenic" is used in many ways, mutually understood by psychiatrists of similar experience, but not suggested by its dictionary definition as a "diagnostic syndrome." Thus, we are compelled to use an inadequate vocabulary, established in the early days of diagnostic classification, to discuss psychiatric material from the essentially dynamic, physiological and adaptive viewpoint that guides us today. Dr. Adolf Meyer, recognizing this many years ago, attempted a sort of psychiatric Esperanto which would give new names to the basic concept of personality adjustment, as "reaction types." Unfortunately, this artificial language has come into daily use only among his closest professional associates; but the value of his effort clearly indicates that a satisfactory technical language, one that naturally expresses the relation of basic assumption to empirical detail, does not exist in psychiatry. Similarly, Dr. Nolan Lewis, in "Research in Dementia Praecox," forcibly called this archaism of psychiatric utterance to the attention of his professional colleagues.

This is a problem for which Dr. Angyal has sought a solution. He despairs of mere clarification and improvement of efforts to interpret

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* Dr. Angyal denies this similarity of concepts but, I think, it is because he does not fully take into account the emphasis of Freud, and his pupil Karl Abraham, on the "oral" libido as the force impelling the human to participate in the life of others. According to the psychoanalytic theory, this basic drive is first aparent in the infant's need to make mother a part of him, and later becomes the mature desire to be a part of the community. It might be said that this aspect of the libido theory provides the ontogenetic basis for what becomes a "trend to homonony."

new trends in the language of discarded concepts. He courageously seeks his solution by going to the roots of the matter, making explicit, by the invention of a new vocabulary, his basic assumption of the interrelations of personality, somatic function, and environment. He frankly states: "A set of new concepts which are appropriate to the holistic character of personality must be developed to permit discontinuance of the use of inappropriate concepts borrowed from the segmental sciences."

Tonight, I seek to show how psychiatry grew up, the path of scientific development that it has traversed within this present century, and the evidence of underlying trends that will determine its future course and its relation to other fields of human study. However, I rather think those more conversant with other sciences can point out that these remarks are just as true of their respective fields, and that psychiatry is blazing this trail because, today, in common with them, it finds the cultural atmosphere in which it may breathe, and grow, and thus gain strength.

We shall, therefore, utilize the recent development of psychiatry to illustrate how this emergent concept of the inevitable dynamic interrelationship of all parts to each other, and to the whole, underlies new techniques based upon the study of thought and emotion. Our effort is to suggest how such a basic concept is implicit in the development of one branch of the humanistic sciences.

DEVELOPMENT OF PSYCHIATRY

The history of any science bears a striking resemblance to certain phases of a growing organism. The growth of a certain part, for a time, accelerates and then retards; but the whole progresses. Each science goes through phases in which its data can no longer be fitted into the mold of universally accepted theories, and phases in which theory advances so rapidly that the hue and cry is raised for scientists to stop speculating and seek only the facts. Today is a day in which the arms of science, its fingers, like a baby's, clutching at the facts, seem longer than its legs.

For historical simplicity, we may date the birth of modern psychiatry at about 1900, and assign the midwife's role to Emil Kraepelin. At that time, psychiatry was, as it is now in less progressive circles, the study of insanity, and regarded as a severe derangement of the brain—a department of neurology. Building up such predecessors as Kahlbaum, Kraepelin succeeded in establishing a classification of his psychotic patients in terms of how they thought and how they acted. This classification of the psychoses has long been accepted as fundamental and generally used for two generations. It provided psychiatry with descriptive categories, based upon factual similarities and dissimilarities, and thus established the descriptive foundation that is essential for the growth of every science.

For some time, it was considered that this infant would grow into a science of the "Specific Cause." But, the aunts and uncles who sought to justify his existence by seeking a specific cause for each type of psychosis in the pathology of one organ or another, or else in a factor in the germ-plasm which made the psychosis inevitable from conception, lost interest. As he grew more lusty and prodigal, he became generally known as "Dynamic Psychiatry," signifying that the symptoms studied by psychiatry may be profitably regarded as end results of the intrinsic and extrinsic forces that determine the adjustment of the individual, his happiness, social values, and health.

Kraepelin's clinical reports show that he was more aware of the emotional factors in psychosis than his descriptive classification alone would suggest. But it was really this baby's first nurses, Eugene Bleuler of Zurich and Adolph Meyer in America, who strengthened his vigor. Bleuler's contribution is symbolized by his translation of Kraepelin's term "dementia praecox" into "schizophrenia." By doing this, he affirmed the diagnostic entity defined by Kraepelin but denied the implication that its victims were suffering from inevitable, progressive deterioration of the brain. To him, "schizophrenia" meant that the bizarre thinking of these psychotics was a dynamic functional process (one way, however alien to social acceptance, in which a brain may naturally function), not merely the mental refuse of a damaged, deteriorated organ. Very much as decerebrated animals reveal underlying motor patterns that are inhibited by a healthy cortex, not newly created by cerebral disease, so, to Bleuler, schizophrenia reveals modes of thought that are latent in all of us, apparent in dreams, but submerged by the rational processes of the healthy adult mind. The mode of thought that dominates psychotics, healthy dreamers, and some mental activities of primitive tribes and infants, was called "autistic thought." This was regarded as the more spontaneous elemental type of thought that the brain produces, less effected by education and less modified by experience than the common sense that assists a man to estimate the outer world and mentally fraternize with his fellows.

As the babe that Bleuler nourished in its earlier years grew to childhood, it came largely under the care of his American contemporaries. Soon after 1900 a small group of these new thinking psychiatrists, of whom Adolf Meyer was the greatest, developed here. Under them, in a few medical centers along the Atlantic seaboard, between 1900 and and 1930, there developed a new and empirically founded conviction in the purposefulness of psychological study and therapy, supplemented by investigation of the social factors. They not only proved the value of their work by founding a new psychiatry, but gave an inspiring impetus to future development. Especially, they fully validated the concept of psychosis, and other mental and emotional disorders, as patterns of adjustments to situations that an individual could not cope with more adequately.

But the position that psychiatry attained in the family of sciences was not alone determined by advances in study and comprehension of mental disease, its etiology, and its therapy. The science it created, and the promise of its future contribution to human knowledge, rests upon a still more fundamental fact than its pragmatic achievements. This fact is the recognition of the value of the empirical study of thought, and especially the practice of the principle that the study of thought could yield an understanding of diseases. Ideas have, of course, intrigued the casual human mind, as well as that of scholars, throughout the ages. But the logic and metaphysics of pre-scientific philosophy was concerned chiefly with the laws of reasoning, and the fact that only a fraction of mental processes are so organized was largely disregarded. In scientific work, thought had been considered only as a *tool* for the investigation of natural processes; from the medical standpoint, the pathology of thought was assumed to be *a priori* proof of organic brain disease of unprovable nature; but, in modern dynamic psychiatry, we find the first body of scientifically oriented professionals taking for granted that thoughts may be as properly considered raw empirical material for scientific investigation.

A statement such as this may astound those who have not previously pondered it. Its value is so easy to take for granted, it may easily be ignored. Actual experience, and the controversies that dynamic psychiatry is still engendering, show that the study of thought is still considered a rather disreputable and unworthy object of the scientific mind. But clinicians cannot so generally pick and choose as experimentalists those studies whose results they may hope to be irrefutable. They have to acknowledge the importance of a process, whether they may hope to explore it with unimpeachable accuracy or not. Physicians could not ignore the existence of thought, but, before Bleuler, they were compelled to treat it as only a by-product of the organs that they understood, rather than as data for studying their most important functions.

Psychoanalysis, as well as dynamic psychiatry, applied the principle that spontaneous thought, especially phantasy, is a proper object of scientific investigation. But the material selected, the method, and the results of psychoanalysis constitute a contribution of its own. Whereas Kraepelin, Bleuler, and Meyer were the greatest members of small groups of men who had jointly discovered new trends of thought that were already emerging, the greatness of Sigmund Freud was, to a phenomenal degree, a single man's achievement. Freud's contribution was, in large part, the product of a mind that opens up entirely new vistas of human knowledge. The existence of an unconscious portion of the mind, in the sense of mental processes that produce symptoms and behavior though the individual is unaware of their existence, had already been demonstrated by Freud's great teacher, Charcot; but Freud not only showed that such things occur in the clinic, but that they are a constant factor of human life and provide the explanation of neurosis. Charcot showed that an unconscious mental process could be activated by hypnotic suggestion. Freud explored what were the ideational elements of the unconscious and the basic laws that controlled their association.

The most important of all Freud's achievements was the invention of a method that, in the course of time, he and his associates developed into a difficult but effective technique. From our perspective today, we can sort out the most fundamental facts that this method has contributed to the studies of science. It has demonstrated that neurotic symptoms, neurotic behavior, and neurotic character, are all adjustments to emotional problems. Emotions represent the need of the individual for certain types of expression, and psychoanalysis proved that in maladjustments of any kind an "emotional conflict" exists, either between a desire that one is conscious of and one that is repressed, or between two unconscious motives.

The second great contribution of psychoanalysis was in exploring the relationship of both normal and abnormal features of the present adjustment of an adult to childhood and infantile experience. For it was shown that phantasies, emotionalized incidents, and patterns of conduct in childhood are perpetuated in the unconscious mind of the adult and affect his behavior and relations with other people, even when these are denied by adult consciousness. This leads to an entirely new perspective of the emotional factors in personality development, character formation, and social adjustment.

There is still another contribution of psychoanalysis, less familiar than these because it has rarely been explicitly evaluated. Yet, to me, it is one of the most significant, especially in the light of those biopsychological trends today, which we shall later appraise. Its essence is expressed in one word used a few times by Freud—*Triebrepresentanz.* This may be translated "instinct-representation"; instinct being defined in the psychoanalytic sense of drive, or emotional impulsion, to a specific form of gratifying behavior. "Instinct-representation" means that thoughts, especially unrationalized thoughts, such as phantasies, behavior, and symptoms, are to be considered mental representations, or indicators, of a biological need. And what is explicit in the word "instinct-representation" is implicit in all of Freud's writings upon the instinct-theory: namely, the basically biological thinking of this master psychologist. This fact is often completely overlooked or denied, because the empirical data upon which his thinking was founded were chiefly psychological. Yet nothing could be farther from the truth than the notion that Freud believed mind to have existence independent of the physical processes of the human organism. His first important papers were on neurological and neurophysiological topics, and his last basic book, "Beyond the Pleasure Principle," went extensively into biological speculation.

But the instinct theory itself is probably the most important conceptual means yet devised for reconciling studies of the mind and studies of the body. Though often criticised and repudiated on first acquaintance, the test of time has proved its lasting value as a conceptual tool. It is a hypothesis, arrived at inductively from psychological data, that is practically identical in meaning to Cannon's formulation of homeostasis from physiological data, in 1926. For, like the theory of homeostasis, it is based on the induction that the human being always functions in such a way as to maintain or restore a certain level of equilibrium:—homeostasis, according to Cannon; instinct tension, according to Freud. "Instinct" is defined as a need that disturbs this equilibrium, produces tension, and thus drives the organism to perform in a certain specific way, called the "goal" of an instinct, that will restore a state of minimal tension, and, thereby, yield psychic pleasure. These goals of instinct are represented mentally by phantasies, often unconscious. But the "source" of instinct is regarded by Freud as a biological product that produces the instinctual need. Even in his first crude outline of this theory, long before our present knowledge of hormones and the autonomic system, Freud remarked, that some day, when chemistry had provided empirical knowledge of the biological source of drive, the instinct theory would no longer be necessary. Thus, an image of food, whether consciously focused or very vague, is regarded as an instinct-representation: the instinct is hunger; its source is the tissues' need for nourishment; and the goal is that food whose ingestion will yield pleasure through restoration of a physiological equilibrium. Psychoanalysis consists essentially of a technique for analyzing far more complex phantasies than a mere image of food, in order to identify these goals and often to demonstrate the relationship of these goals to earlier experience and social relationships.

Thus, very broadly, very schematically speaking, psychiatry, as it is today, resulted from the confluence of two great streams: one has been called dynamic psychiatry; the other, psychoanalysis; both of them based upon appreciation of psychological data as the empirical base of certain medical studies. An interesting historical speculation is how much Bleuler's impetus to the development of dynamic psychiatry in America was influenced by Freud. For it is a fact that Bleuler, with several other Zurich psychiatrists, constituted the first group, in 1902, to recognize the value of Freud's work, and to apply his ideas and technique in their own clinic. Another of this group, Carl Jung, made the first intensive study of schizophrenia from Freud's standpoint, in 1907, and a third, Max Etingon, eventually, in 1921, established in Berlin the first systematic training for the practice of psychoanalysis. But Bleuler did not become a psychoanalyst, possibly because he was chiefly interested in the psychoses, whereas Freud studied chiefly neurotic problems. At any rate, after a few years, the course of dynamic psychiatry and psychoanalysis diverged. It was as though the common river, resulting from two streams which flowed together, and fed by many smaller tributaries, was split by a lengthy island into two channels, one narrow and turbulent, the other broad, and interrupted by sandbars and shifting shoals. Eventually, the two currents met again in the American psychiatry of the 30's.

For much as the average European would not appreciate how vast is the water drained by the Missouri River into the more famous Mississippi, so is it rarely understood how much the psychiatric current of the last ten years is a confluence, after twenty years of division, of the dynamic psychiatry developed most extensively in America by Meyer, his colleagues, and their students, and the psychoanalysts who studied in Europe. Subsequent events have shown that not only the early American analysts, Putnam, White, and Brill, but their younger successors in the 1920's, were sowing scientific seeds they had garnered in Europe, but sowing them in a different soil. The cleavage between university psychiatry and psychoanalysis was much more complete in Europe, and the hostilities more violent. In part, this was due to the basically organic concept of psychiatry which, except in a very few centers like Zurich, made the appreciation of a dynamic psychological approach much rarer, compelling the first generation of analysts to work in isolation. And, in part, it was a consequence of the impassioned monasticism, and the intensely patriarchal atmosphere, which developed among analysts themselves and produced among them a contemptuous hostility to their colleagues. Thus, the first generation of European analysts regard psychiatric training as worthless and often harmful to an analyst; but American analysts felt general psychiatry, as they had learned it in our best clinics, had been essential in their training, and, so, required it of their students. Whereas most European analysts at that time considered psychosis universally inaccessible to psychotherapy, this group of Americans had proved with their personal experience that this was not always so, even before their analytic careers began. Whereas a majority of Europeans, including Freud himself, believed laymen were as well qualified as physicians for the practice of analytic therapy, Americans considered analysis to be, properly, a specialty of the medically trained.

Not only did general psychiatry play a recognized and valued role in the development of analysts here, but analysis also more profoundly affected psychiatry than in Europe. William Allen White especially insisted upon the importance of its application to research and therapy in mental hospitals. In the last fifteen years more trained psychoanalysts have been prominent on the teaching staffs of our medical schools, not as instructors in the special technique of analysis primarily, but as teachers of psychiatry. Many important analytic words are now used as naturally in the everyday lexicon of the modern psychiatrist as in the psychoanalytic seminar. He may still attack analysis in his speeches, but in his ward rounds he refers, as a matter of course (not stopping to think of the foul source from which these useful terms are drained), to the "unconscious," "repression," "conversion," "character pattern," "defense reaction," and many other ideas produced by psychoanalysis.

Psychoanalysis is, therefore, still a specialty, but its material and inferences are being more and more assimilated into the thoughts and procedures of other psychiatrists and medical practice. Its role in the amphitheatre of medicine is resembling more and more the role of pathology that William Henry Welch introduced after his return from his studies in Germany. At first, he found himself in possession of a body of knowledge little understood by his colleagues and not considered essential to the day's work of the practicing physician. Pathology remained a specialty to which many individuals devoted, exclusively, their research lives. But, while maintaining its identity, its contributions came to be recognized as the fundamental basis of scientific clinical work. Psychoanalysis seems to be rapidly attaining a comparable position among those techniques that are based, in whole or part, upon psychological data.

Within the last decade, a third vital tributary has joined with dynamic psychiatry and psychoanalysis in developing the applications of these techniques. This is *psychosomatic medicine,* a term used to designate that department of medicine that studies the relationship of emotional problems to diseases that involve pathological changes in structure and functions of the body. It is based upon the principle that emotional tensions, chronic or acute, are often discharged in ways that upset the normal physiological equilibrium of an organ, thus initiating the somatic changes that produce disease. Such factors as social, neurotic, and psychologically traumatic tensions have long been vaguely recognized as significant, but only recently have we begun to study them intensively. Perhaps medicine had to await the development of modern psychiatric techniques before the "clinical hunches" of the intuitive internist could be built into promising research projects. The mind that first clearly perceived these great problems in terms of modern medical and psychiatric experience was that of a New Yorker, Smith Ely Jelliffe. His very early contributions to this field, beginning in

1909, constitute, by all odds, America's most original application of psychoanalysis. But, partly because his theoretical inductions were not as sound as his clinical acumen, and partly because his colleagues, both analysts and internists, were not yet prepared to recognize the significance of his efforts, psychosomatic medicine, as an expanding field of intensive research by many workers, is a very recent thing.

This accelerated interest of the last ten years has, however, come as much from without as within the psychiatric specialties. The family doctor of a generation ago, who knew so many of his patients from the cradle to the grave—sometimes knowing his patients better than their diseases—took for granted the fact that different people and different experiences of the total human being were major factors in their health and sickness. Even an age of intensive specialization has not blinded the practicing clinician to this basic fact, though it has puzzled him, and he has found it impossible to reconcile his experience of human nature and his knowledge of scientic postulates. Over and over, it has been pointed out that certain diseases, for example, gastric ulcer, hypertension, exopthalmic goitre, dysmenorrhea, and sterility, are closely correlated with emotional crises and tensions. To those who have given them careful study, there is little dispute that neurotic problems may be resolved through nerve pathways that initiate these pathological syndromes.

Thus, the increasing tendency of internist and psychiatrist to have a common basis of interpretation in principle, if not in practice, is rapidly putting an end to the traditional differentiation of "functional" and "organic." This time-honored basis of differential diagnosis and therapy, this arbitrary criterion for distinguishing cases in the psychiatrists' field from those which aren't, is no longer compatible with the modern psychosomatic view. The advance of neurophysiology has added conviction and impetus to the clinician's need for psychosomatic study. In particular, the newer knowledge of the functions of the autonomic nervous system has bridged the chasm between those who studied body and those who studied mind. We have shown that the study of phantasies, which represent emotional needs of the human being, was clearly recognized by Freud; but, today we know that emotion itself is the mental reflection of physiological events within this autonomic system. Thus, dynamic psychiatry might describe an example of neurosis as a maladjustment to social demands; psychoanalysis, as the result of an emotional conflict preventing adequate discharge of instinctual tension; while, today, the organicist would state that certain tensions within the autonomic system were discharged through disease-producing innervations. It turns out that they are all referring to the same process, but each has studied the process with different types of data: one expresses it in the language of social psychiatry; another in the language of unconscious conflict; another in terms of the physiology of the autonomic system.

Our newer techniques, crude and inaccurate though they are, none the less represent a genuine application of the holistic principle. It is rarely appreciated that the best psychological methods of psychiatry are, in a very fundamental sense, techniques of physiology, if they are based upon the assumption explicitly stated by Freud that ideas are representations of biological forces. Thus, they are samples, not of a single chemical component of the blood, not of a single nucleus of the brain, not of a single normal or abnormal organ function, but samples or indicators of the biological organism reacting as a whole to that which drives it from within and confronts it from without. Psychological data are, therefore, truly physiological data in the sense of disclosing those reactions of the organism that are too complex and too subtle to be studied by other means.

This is the basic reason why psychoanalytic technique has been called newly into service for psychosomatic research. And it is, I believe, even though not usually formulated in the medical philosophy of the day, the reason why medical relations with psychiatry are changing so rapidly and so profoundly, even within a period of five years.

By this time, I may seem to be so possessed by my specialty as to imply that psychiatry will usurp or displace the other great branches of medicine. That, indeed, is far from my intention. My thesis is, rather, that psychiatry, from the very nature of the material with which it deals, illustrates and, to some degree, anticipates a trend of culture and of science whose effects are more profound than any single department of medicine, or of any science.

This underlying, dynamic trend, affecting and propulsing the development of all, first came to my attention years ago. I had dinner with an organic chemist and, to my amazement, had listened to his youthfully impassioned story of professional troubles. He was enraged at a series of lectures he had recently attended. A world famous physical chemist had traveled across the continent to deliver them before the organic chemists at Harvard. The topic had been the chlorine atom.

My friend, incensed by the ignorance of this authority in applying his knowledge to organic chemistry, had discoursed throughout the evening on what the fellow did not know. Neither the precision of his method nor the validity of his data were disputed; his conclusions were proved. Yet—the properties he defined were incompatible with what was known by those who had studied chlorine in complex organic molecules. The physical chemist was limited by his devotion to finest quantitation. He was blind to the significance of less exact methods and contemptuous of data that the student of such complex things as the organic molecule was compelled to use.

Though I knew little chemistry, the tone and thesis of my friend's complaints were all too familiar to me, and I was astounded that they should come from the intellectual heart of a man devoted to laboratory research. It had never occurred to me in my professional youth that the headaches of a chemist could so resemble those of my profession. How similar to my feeling about the reactionary psychiatrist who assumed that, by curing a hysterical symptom, he had affected the basic neurosis or improved the patient's life; or my reaction to physicians who, constantly confronted with what they never studied, considered every neurotic trait a nuisance to be gotten rid of and not as one kind of suffering and poor health to be studied and treated! How often I had railed against the physiologist who, preoccupied with his demonstration of the role of adrenalin in anxiety, denied the relevance of the question he could not try to answer: why one man's adrenalin went up when he saw a horse, another's when the lightning crackled, and another's when he was left alone with a woman!

The organic chemist had therefore recognized the importance of the complex molecule, and, in studying it, sacrificed temporarily the ultimate goal of science,—exactitude and irrefutability. And we have seen how similarly psychosomatic medicine has developed from the recognition of the role of the total personality, to say nothing of the biosphere, in diseases only single elements of which can be isolated for exact examination in the laboratory.

Are these not examples of a trend which is transforming all sciences today? In the field of biology, ecology has attained a distinguished place within a generation. Psychology has progressed from study of sensory experience to the highly integrated functions examined by psychometrics, and to the even more complex material of Gestalt, genetic, and some experimental psychology. In sociology, Pareto may be cited as the supreme example of what that science will some day owe to the recognition of irrational motives as the primary source of social institutions. Many important details of Pareto's theories duplicate indeed those of psychoanalysis, and his individualized vocabulary is easily translated into that of Freud. But, perhaps, the most striking example of all is that of the new science of semantics. We do not need to argue with Korzybski's assumption that psychosis results from the peculiar use of words, and discard all other psychiatric knowledge that people who use words pathologically are adjusting to problems of the total organism. But that words are used with special meanings, multiple connotations, individually determined referants, is an everyday clinical observation by dynamic and analytic methods of psychiatry. The demonstrations of Richards and Ogden, that dictionary definition is just one of many meanings a word has for the speaker and the hearer, are just as truly an analysis of unconscious meaning as the work of Freud on other aspects of human thought; just as truly the beginning of a scientific approach to the study of ideas.

In all these fields of endeavor, one sees the basic, science-changing trend to supplement precise study of the detail by less exact, but no less searching, and critical investigation of the larger unit.

CONCLUSION: A PREDICTION

The concept of psychiatry which is emerging as dominant in our present generation is, consequently, what Dr. Angyal calls the "biosphere," in order to express the idea that "organism and environment are two indispensable poles of a life process." This is an idea which twenty years from now may not be named or much thought about because it will be taken for granted. It involves the assumption that psychiatry offers those techniques that are useful for investigation of the normal and abnormal manifestations of the total human organism, and the relationship of these to the social role of the individual, to his mental and physical health, and to the forms of disease to which he is latently or manifestly susceptible. Other disciplines, medical, biological, and sociological, will be far more intrinsically interrelated with psychiatry than in the past. Psychiatrists will be, as a matter of course, internists and sociologists; and, to an even greater extent, allied disciplines (biology, physiology, pathology, anthropology, and genetic psychology) will take for granted and apply the holistic premises that were born with methods for studying unrationalized ideas and behavior.

If this prediction proves correct, and such a development actually comes to pass, it will be only logical—nay, it seems to me, the inevitable, unfolding of science in general, and psychiatry in particular. What cannot be predicted is a great new idea, the impact of such a mind as Claude Bernard's, or Darwin's, or Freud's upon a generation, somehow unpremeditatedly prepared to grapple and to extend the super-prescient product of such a mind. To envisage such a future landmark would be futile. But, may we dare to hope it will solve that basic problem of psychiatry today, the lack of an accurate technique for quantitating the things we study? We seem to have grown beyond the need merely to imitate sciences more exact, to flee to techniques that really were not psychiatric at all, but those of laboratory pathology and chemistry, or very frequently to put on that torn and tattered coat of the scientific pretender, a column of figures. But a truly quantitative measurement of observations is the hallmark of scientific maturity. Less than a hundred years ago, Claude Bernard laid the foundation of physiology. We see this science now grown up, so we often forget that the work of this great man was largely qualitative. In the words of Olmstead, his biographer, "Bernard deprecates rashness in the application of quantitative methods to the undeveloped science of physiology." And then he quotes directly from the *Introduction to Experiment and Physiology* (1865): "The application of mathematics to natural phenomena is the aim of all science, because phenomena should always be mathematically expressed." "But," continues his biographer, "Bernard thought that mathematical deductions were being made in his day on the basis of situations too crude for the results of calculations to be valuable."

A rather exact parallel is presented by psychiatry today, and its inexactitudes are no contradiction of the importance of the qualitative knowledge it has achieved. As well despise the babe because it is not yet the man; ignore the potentiality of future development that its vigorous kicks and squawks reveal!

And so, tonight, we have recalled the delivery of this babe, in Kraeplin's clinic, from the mysterious womb of supernaturalism, in which he had been cherished throughout the ages. We have surveyed the development of this younger member of the scientific family, surprisingly rapid as it always is in the earlier stages of growth, and we have seen its emergence to the dignity of the tortured adolescent. If Doctor Angyal is correct, someday it may live up to its newly acquired long pants!

SECTION OF ANTHROPOLOGY

NOVEMBER 22, 1943

DOCTORS PAUL F. LAZARSFELD, and ROBERT K. MERTON, Columbia University, New York, N. Y.: *Studies in Radio and Film Propaganda.** (This address was delivered by Doctor Robert K. Merton.)

This is a report on certain studies of domestic propaganda in radio and motion pictures. Having said this, let us define the term "propaganda" and let us make the definition hold throughout our discussion. We understand by propaganda any and all sets of symbols which influence opinion, belief or action on issues regarded by the community as controversial. These symbols may be written, printed, spoken, pictorial or musical. If, however, the topic is regarded as beyond debate, it is not subject to propaganda. In our society, the belief that 2 and 2 make 4 cannot, in this sense, be propagandized any more than the moral conviction that mother-son incest is evil. But it is still possible to propagandize the belief that our victory in this war is not inevitable; that the poll tax runs counter to certain conceptions of democracy; that it would be unwise to provide citizens with as much fuel oil and gasoline as they wish; that one religious system has greater claim to our allegiance than another. Given a controversial issue, propaganda becomes possible and, it would almost seem, inevitable.

Another general remark. In many quarters, propaganda is often identified with lies, deceit or fraud. In our view, propaganda has no necessary relation to truth or falsity. An authentic account of sinkings of American merchant ships may prove to be effective propaganda inducing citizens to accept many deprivations which they would not otherwise accept in good spirit. If we succumb to the view that propaganda and falsity are one, we are well on the way to nihilism. Let us recognize also that an attitude of uncritical distrust may develop as a defense against the acceptance of deprivation or against a barrage of facts and information which invite fear, discomfort or the abandonment of cherished beliefs.

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* In essentials, this paper was originally presented by one of the authors at the Writers Congress in Los Angeles, October 1-3, 1943. The proceedings of the Congress will be published by the University of California press. The authors are grateful to Mr. Marc Connolly and Mr. Ralph Freud, Co-Chairmen of the Congress, and Mr. Franklin Fearing, Chairman of the Propaganda Panel, for their permission to publish the material in the TRANSACTIONS.

But it is long since time to halt discussions of propaganda in the large; discussions which have all the fascination of speculation uncontrolled by empirical inquiry. To bring certain problems of propaganda into clear focus, we must turn to propaganda in the particular, and develop definite procedures for testing our interpretations. It is not that general discussions of propaganda are necessarily invalid; it is only that they tend to outrun our funded knowledge. They are big with the bigness of vacuity.

Possibly this paper errs in the opposite direction. We intend only to report some of the recent studies conducted in the Columbia University Office of Radio Research under the supervision of the authors and Dr. Herta Herzog. One characteristic of these studies is their concern with the ascertainable effects of particular propaganda documents. Another characteristic of these studies is their technical orientation: they constitute one basis for advising the writers and producers of this propaganda. The research must be such as to implement immediate decision and action. A dozen years before he fled to Samoa, Robert Louis Stevenson was unwittingly describing the very type of situation which confronts research students operating within the framework of political action:

> This is no cabinet science, in which things are tested to a scruple; we theorize with a pistol to our head; we are confronted with a new set of conditions on which we have not only to pass judgment, but to take action, before the hour is at an end.

The present report, then, deals with research conducted "with a pistol to our head." Our object tonight is to plead that you not press the trigger.

In one sense, detailed propaganda analysis is not a new development. For at least the past decade, the effects of films, radio programs and newspaper materials have been studied. As a rule, however, these studies have dealt with the over-all effects of the propaganda material as a whole. These researches—for example, those of L. L. Thurstone—have consequently confined their general results to observations of this order:

> An anti-Negro film, "The Birth of a Nation," increased anti-Negro sentiment among tested audiences.
>
> The film, "Streets of Chance," which portrayed a gambler "as an interest-

> ing, likeable character," for some unascertained reason led to an increased condemnation of gambling.
>
> The film, "All Quiet on the Western Front," led to more marked reactions against war among groups of school children than did the film "Journey's End."

You will notice that such research tells us little about the specific features of the propaganda which provoked these effects. But this is the very question with which the script-writer and the producer are concerned. If they are to benefit from propaganda research, it must be directed toward discovering the typical effects of definite and specific aspects of propaganda as well as its over-all effects. What is the character of effective propaganda under given conditions? In this paper, we shall report samples of our recent studies in which definite features of propaganda are linked with definite types of response.

Before turning to methods of analyzing propaganda effects, we should seek to dispel one common illusion. It is clear that, in general, writers of propaganda cannot know how audiences will respond to their material merely by relying on intuition or by observing their own reactions. Several examples, the first of which is educational rather than propagandistic, will illustrate what unexpected responses the writer may elicit.

A skilled writer had drafted the instructions for the use of the second war ration book in as lucid a fashion as he could. Psychological consultants assisted him in his task. Trained interviewers presented the instructions to housewives and observed their reactions. On the basis of these observations, a second draft of instructions was prepared. This also was tested by interviewing, and a modified third draft was finally adopted. A central objective was that of making it clear that ration stamps of different values could be added to reach a given number of points.

It was assumed that since most people have had experience with postage stamps, an analogy might profitably be used in the instructions. Who would have anticipated from the vantage point of his armchair that this simple analogy would elicit comments such as these:

> I didn't realize that you had to mail them.
> There doesn't seem to be any place to stick them.

This trivial example of the unexpected response merely reflects a breakdown in communication. Other illustrations are provided by films which emphasize the cruelty and immorality of the Nazis. Episodes which ostensibly indicate that the Nazis are entirely unconcerned with common human decencies are at times appraised by audiences in purely technical terms: they are taken as illustrations of Nazi efficiency. The emotional and moral implications intended by the producers of these films are overlooked by the audience.

Much the same pattern of the unexpected response is found in radio materials. A talk on X-rays was recently broadcast under the auspices of a medical society, as part of a campaign seeking to promote "proper" use of health services by members of the community. The speaker, a noted radiologist, attempted to dissuade his listeners from turning to unlicensed practitioners (quacks) for X-ray examinations and treatments. In an effort to make his persuasion effective, he repeatedly stressed "the dangers in the use of and in the making of X-ray examinations." The radiologist's good intentions elicited unexpected anxieties. Some members of the audience—who, in any case, would not have consulted quacks—expressed their newly-acquired fears:

> It left people not wanting X-rays. It sounded so dangerous. The doctor uses lead and wears gloves. People wouldn't even want to get an X-ray after that. They'd be scared away.
>
> I would feel that maybe it would hurt. From hearing about currents and so on I would think that it would be at least unpleasant.

The pattern of the unanticipated response raises several basic questions. How can we analyze propaganda in films, radio and print, in such a way that we can determine what is likely to produce given effects? The procedures for achieving this end have come to be known as *content-analysis*. There are further questions. How can we determine responses actually elicited by propaganda? How far can we account for discrepancies between anticipated and actual responses? Can we build a fund of experience and interpretation which will enable us more fully to anticipate responses to various types of propaganda, thus minimizing or precluding undesirable responses by appropriate modification of the propaganda before it is released? Procedures designed to answer these questions we shall call *response-analysis*.

And now we turn to what we consider our main task: to report our experience in the analysis of various types of propaganda during the past two years. Perhaps by focusing on problems actually encountered in these studies, we can make clear some of the procedures of content- and response-analysis which have been developed.

CONTENT-ANALYSIS

The propaganda document—a pamphlet, film or radio program—is first scrutinized to determine the probable types of responses to its various components, aspects, or to the document-as-a-whole. It may be assumed, perhaps, that anyone who examines the propaganda material will know its content. But this is far from being the case. Content-analysis requires certain procedures, based on clinical experience and funded in psychological or sociological theory, in order to discern the possible responses to the content. Mere impressionism is not enough. The content of a 15-minute radio program or of an hour film can be adequately appraised only through systematic procedures. Just as we need a field glass to perceive an object in the far distance, so we need devices, at times surprisingly simple devices, to perceive a flow of experience which endures over an extended period of time. These devices, which can only be briefly described within the compass of this paper, vary from the one extreme of counting the frequency of certain *key symbols* to the other extreme of determining the *structure* of the propaganda-as-a-whole or of an entire propaganda campaign.

Let us consider a few examples of the simplest type: symbol-analysis. A radio series of morale programs contained approximately 1000 symbols denoting the United Nations (or its constituents, other than the United States) and the Axis (distributively or collectively). Upon examination of the frequency of these respective sets of symbols in twelve programs, several uniformities emerge which reflect a structure of the programs running counter to the manifest intent of the producers. In all but one of the programs, the frequency of United Nations symbols is positively correlated with those pertaining to the Axis: an increase or decrease in the one set of symbols is associated with an increase or decrease in the other. This brought to the fore a significant pattern in these morale programs. Interest in the United Nations is largely confined to their role in the war vis-à-vis the Axis: they are seldom mentioned in any other connection. So far as this morale series is concerned, the United Nations appear to be "foul-weather friends": interest is primarily manifested in them as allies helping to fight the Axis, and not as allies with whom we have sympathetic ties, irrespective of the war. The programs deal with them, not as societies, but only as nations exhibiting military prowess and courage. We salute the heroic dead of the Russians and rejoice that they are enemies of Hitler. We eulogize the British who have so long held the fortress Britain against the Nazis. Or, we mourn the fate of the occupied nations and, again, interest in these nations is limited to their experience at the hands of the enemy. Because these are the motifs expressed in allusions to the United Nations, we find the observed association between the frequency of symbols referring to the United Nations and to the Axis. It should be noted that the analysts, and possibly the producers, of this radio series would not have detected this underlying structure had the symbol-counts not called it to their attention.

This series of programs also made extensive use of the "personification stereotype" in referring to the enemy: about 25 per cent of all symbols denoting the enemy refer to Hitler, Mussolini, Goering, *etc.*, whereas only four per cent of references to the United Nations and 11 per cent of those to the United States consist of personifications. This use of simplified personalized stereotypes presents the enemy as consisting essentially of a small band of evil men and implies that once these men are destroyed, all will be well. This kind of personification proves to be all too acceptable to listeners, since it accords with common simplistic ideas: for example, the parallel notion that we must fight crime primarily by punishing criminals and not by preventive measures.

Moreover, we have found that varying distributions of terms used to designate the enemy in documentary films are reflected in the comments of interviewees who have seen these films. Thus, if the single satanic figure, Hitler, or the entire German people, rather than the Nazis, are most frequently identified as the enemy by the film commentator, this is reflected in the pattern of audience responses. We need only remember reactions to the war-guilt clause in the Versailles treaty to realize that the issue has considerable political importance. Current propaganda may be inadvertently ignoring the Fascist or Nazi character of the enemy and thus building up a reservoir of misdirected ill will for the post-war period.

Another example is provided by a pamphlet concerning Negroes. The main themes of the pamphlet, as you may remember since it was publicly distributed, were two: It is true that Negroes continue to suffer from discrimination but, none the less, they have made great progress in our democratic society which has enabled many Negroes to achieve individual success and to contribute to the community. In contrast, Hitler has always expressed contempt for colored peoples and, should he win the war, all gains of the Negro would be wiped out. The content of the pamphlet can thus be classified in two categories: material pertaining to "Negro gains and achievements in a democracy," and to "deprivations threatened by a Hitler victory." There were 189 paragraphs and captions: 84 per cent of these dealt with present gains and 16 per cent with potential losses under Nazism. To the producers of the pamphlet, this evidently seemed a reasonable distribution of emphasis on the two themes.

But the pamphlet contained two types of presentation. One was an article by a prominent Negro writer; the other, a series of attention-fixing photographs with short captions. Further thematic analysis found that the photograph-captions and the article presented the two themes in completely different proportions. Some 73 per cent of the items in the article referred to losses under Hitler and 27 per cent to gains in a democracy, whereas 98 per cent of the photographs and captions referred to gains and only two per cent to the Hitler threat.

Now it so happens that a majority of the population, and particularly the Negro population, in general, prefer photographs and captions to a detailed text. They are more likely to look at the former than the latter. The photographs, in this case, almost wholly neglected the theme of Negro losses in the event of a Nazi victory. As a result, the pamphlet largely missed its mark. Certain attitudes of Negroes were tested both before and after they read the pamphlet. Most of the readers experienced pride and a higher ego-level as a result of this testimonial to the achievements and contributions of the race. But the pamphlet failed to canalize special motives for Negroes to push the fight against Nazism in their own interest, since readers had largely overlooked the essential message.

However cursorily, these two examples illustrate ways in which ordinary counts of key-symbols and thematic analysis enable us to discover inadvertent errors of the propagandist. They also serve as a guide to interviews with persons exposed to the propaganda. There are other types of content-analysis which can only receive the barest mention in the time at our disposal.[1] The following brief summaries may, however, be sufficient to indicate their general nature:

1. *Symbol-counts:* Consist of identifying and counting specified key-symbols in communications. This merely indicates, in a restricted fashion, the

[1] See **H. D. Lasswell**, A provisional classification of symbol data. Psychiatry 1: 197–204. 1938; **Douglas Waples et al.**, What reading does to people. Appendix B, Chicago. 1940; **N. C. Leites & I. de Sola Pool**. On content analysis. Experimental Division for the Study of Wartime Communications. Document No. 26. September, 1942.

symbols which have been at the focus of attention of audiences. The count of references to the enemy in film commentaries illustrates this type.[2]

2. *One-dimensional classification of symbols:* This is a slight elaboration of the previous type. Symbols are classified according to whether they are employed, broadly speaking, in positive ("favorable") or negative ("unfavorable") contexts. Thus, Britain may be described in + terms (victorious, democratic, courageous) or in — terms (defeated, caste-ridden, perfidious). This type of analysis is a first step in determining the most effective distributions of symbols for reaching a given result. It may serve to check the often ineffectual practice of dealing in black-and-white contrasts. When applied to enemy propaganda, this kind of analysis provides one basis for gauging the relative security or insecurity of the enemy.[3]

3. *Item-analysis:* Classification of segments or sections of the propaganda (e.g., scenes in a film; songs in a radio program; photographs in a pamphlet). This requires selection of significant and insignificant items on the basis of a psychological theory of "attention-value." Will these items tap central or peripheral interests of the audience? How will these items be interpreted by different types of audiences? In several analyses of films, it was possible to predict scenes and sequences which would be at the center of attention of audiences.[4]

4. *Thematic-analysis:* Classification of the explicit and implicit (symbolic) themes in propaganda material. This, as distinct from item-analysis, deals with the supposed cumulative significance of a series of items.[5]

5. *Structural analysis:* Concerned with the interrelations of the various themes in propaganda. These relations may be *complementary* (enemy is cruel, we are merciful); *integrated* (enemy is cruel, deceitful, aggressive, irreligious); *interfering* (when themes work at cross-purposes; e.g., theme of Nazi strength produces anxiety).[6]

6. *Campaign analysis:* Deals with the interrelations of different documents all of which are designed for a general over-all purpose. Whereas structural analysis deals with the relations *within* a single propaganda document, campaign analysis deals with the relations of a series of such documents. Problems of sequence, duration, relative emphasis, timing, as well as the relations mentioned under structural analysis, are involved.[7]

From this summary, we see that a major task of content-analysis is to provide clues to probable responses to the propaganda. But this is not enough. We must see whether these anticipated responses actually occur, whether our content-analysis is essentially valid. This requires interviews with members of audiences; interviews of a special type, which we shall call the "focused interview."[8]

Incidentally, there is interaction between analyses of propaganda content and focused interviews with readers and listeners. A prior content-analysis is indispensable for helping to guide the interview and experience in interviewing sharpens your eye for more adequate content-analysis.

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[2] See, for example, **H. D. Lasswell**. The world attention survey. Public Opinion Quarterly, 5: 452–462. 1941.

[3] For example, studies by **Hans Speier & Ernst Kris**, Research Project on Totalitarian Communication, at the New School for Social Research; an unpublished symbol-analysis of the "This Is War" radio series, Office of Radio Research, Columbia University.

[4] For example, unpublished studies of documentary films by the Office of Radio Research.

[5] For example, a forthcoming study by Gregory Bateson of a Nazi propaganda film. See, also, **Siegfried Kracauer**, Propaganda in the Nazi War Film. New York: Museum of Modern Art Film Library. 1942.

[6] For example, **Kracauer**, *op. cit.*; also film studies by the Office of Radio Research.

[7] For example, studies of political campaigns, public utility propaganda campaigns, bond drives, etc. See a forthcoming study of the 1940 political campaign, by Paul F. Lazarsfeld.

[8] A monograph on the focused interview is in preparation by Robert K. Merton.

RESPONSE-ANALYSIS

Interviews designed to discover actual responses to propaganda seem, at first sight, a simple task. But in actual experience, it is not so at all. Use of the customary interviewing techniques does not suffice to obtain the needed information. Most people find it difficult to express their reactions to a film or radio program in terms which will be of use to the writer or producer or psychologist.

We have found that respondents fall into two broad classes. If they are highly articulate, they will usually express their advice on how the film "should be presented" or how the radio program "should be revised" to increase its effectiveness. They seek to act as professional critics or consultants, and this is precisely what we do not want. Interview tactics have had to be devised for the purpose of avoiding such "consultant attitudes" on the part of interviewees and of making it possible for them to report their own immediate responses to the propaganda.

For other subjects who find it difficult to report their responses at all, special interview techniques have been developed to enable them to render their experiences articulate. The entire interview is focused in terms of the propaganda material which is being tested. The interviewer's remarks do not direct attention toward definite aspects of the propaganda. They merely facilitate the respondents' reports of their own centers of attention and of their own reactions to these items which are significant for them. If the figure be permitted, the interviewer provides the respondent with a flashlight which illumines the traces of the film or radio program or printed material in the respondent's mind. It is only after the interviewees have fully reported their reactions to the aspects of the propaganda which they experienced most vividly that the interviewer rounds out the discussion by checking those hypotheses derived from content-analysis which have not yet been considered in the interview. The entire interview is recorded verbatim by stenotypists. This permits a later intensive analysis of just what aspects of the propaganda elicited certain types of response.

In general, we may say that a focused interview is valuable according to the extent that it achieves the following objectives:

1. Determines the effective aspects of the propaganda to which the audience has responded.
2. Determines the many-sided nature of these responses in considerable detail.
3. Enables us to test whether the responses which we expected on the basis of content-analysis have actually occurred.
4. Discovers wholly unanticipated responses; that is, responses which were anticipated neither by the writer nor by the content-analyst.

Although all of these objectives of the interview are important, it is the last which is of special practical importance. You will remember our examples of the Negro pamphlet and of the radio talk on X-rays. These were intended to indicate to you that without a content- and response-analysis to aid him, the propagandist sometimes cannot see the forest for the trees. We should suggest, further, that often the propagandist cannot see the thorns for the rose. If a propagandist wishes to convey an idea or create a given impression, he must do it by words, illustrations or other symbols. Once his pamphlet, play, radio program or script is out in the world, it is for the audience to understand him as they will. The story is told of a missionary who pointed to a table and repeatedly said "table" until his audience of non-literates could repeat the word. After some time, he was dismayed to learn that some non-literates referred to a tree as "table," because both were brown. Others called dogs "tables" since both had four legs. In short, each listener had selected some aspect of the complex object, which for the missionary was so well designated as a whole by the word "table." In the same way, it is instructive to see how often the effects of propaganda can be totally unexpected.

The case we want to consider here is derived from the previously mentioned test of a health program. This has wide implications, should the government seek to maintain the educational and propagandistic functions which it has assumed in an effort to maintain morale during the war. Having had the experience of accepting some measure of government supervision, the American population may prove more receptive to the promotion of public health, nutrition and educational activities in the post-war period.

In this instance, it will be remembered, a representative of a county medical society broadcast a talk on X-rays. He stressed the precautions needed to prevent X-ray burns; he indicated that the local government protects the citizen by a system of licensing X-ray operators and by inspecting equipment; he emphasized the specialized training required to attain competence in this field. The speaker was evidently seeking to prevent his listeners from falling into the hands of quacks who have neither competence nor integrity. Professionally concerned with this problem, he apparently did not realize that his listeners had not accumulated experience comparable to his own. He neglected to integrate the problem into the experience-world of his audience.

It is well known from related fields of investigation that listeners cannot readily assimilate information and attitudes if these are not tied into their backlog of experience. Had the physician described the procedures used by quacks for obtaining clients, or had he indicated how they might readily be recognized or, even, if he had presented figures on the presumable number of unlicensed operators in this field, his listeners might have assimilated his views and attitudes. Since he did not, he seemed to be pounding at open doors.

> He talked about licensed doctors but he didn't make it too clear. He never said what would happen to you if an unlicensed person did it.

As a result, listeners began to doubt the importance and, at times, the reality of the issue. The physician talked, as it were, into a psychological void which the listeners had somehow to structure for themselves. They had been told of the complexity of X-ray apparatus, and they used this newly acquired information to look at the problem in their own way.

> I don't think the warning is justified at all. Just anyone can't have an X-ray machine. General Electric probably wouldn't sell the equipment to anybody without a license.
>
> I couldn't conceive that anybody without a license would dare to buy such expensive machinery for about $10,000 only to be caught the next day by somebody who found he didn't have his license.

Possibly intending to meet this problem, the speaker goes on to extol the merits of the specialist in general terms. A content-analysis found 63 references in 14 minutes to the conceptions of authority, licensing, and specialization. Since the talk raises issues which it fails to clarify, it leads to a *boomerang effect.* The listener becomes more and more impatient and in the end challenges the X-ray expert himself.

> There are a good many cases where there is a licensed man and he doesn't use the X-ray just right.
>
> You can get an automobile license but that doesn't prove you can drive. In the same way these people can get a license but that doesn't prove they are competent.

The program stressed the value of proper training for X-ray specialists. But it assumed, erroneously, that listeners had the mental set necessary to identify licensing with appropriate skills. Consequently, the whole emphasis of the speaker led first to impatience, then to disbelief and finally to distrust.

Under certain conditions, then, people respond to propaganda in a fashion opposite to that intended by the author. In the course of our tests, we have found various types of such boomerangs, some of which may be mentioned here. The foregoing "specialist" boomerang illustrates a familiar type: *it results from an erroneous psychological appraisal of the state of mind of the audience.* Propaganda will not produce the expected response unless its content corresponds to the psychological wants of the audience. It is necessary, therefore, to have a continuing flow of intelligence information concerning prevalent attitudes and sentiments in the population, if propaganda is not to invite boomerangs. It is at this point that the familiar types of "opinion polls" and other mass observation studies are linked with detailed propaganda analysis.

We know, for instance, from public opinion polls that a large proportion of Americans believed, at a time when it was not remotely the case, that we had the largest army, the greatest production of war materials and had contributed most to victory over the Axis. Therefore, films which seek to emphasize the contributions of our allies must be especially designed not to feed this ethnocentrism. If we want to show what the British or the Russians or the Chinese have accomplished, sequences dealing with lend-lease aid or other American contributions must specifically and explicitly indicate the limits of such assistance. Otherwise, we shall find the indicated type of boomerang effect, where a neglected psychological set of the audience deflects the film to ends other than those for which it was intended.

A second type of boomerang-effect is probably part of the irreducible minimum of boomerang-responses. *It arises from the dilemma confronting the writer who must address his propaganda to a psychologically heterogeneous audience, i.e. the members of which are in different states of mind on the given issue.* Material which is effective for one segment of the audience may produce opposite effects among another segment which is socially and psychologically different.

Let us take a case in point. A radio morale program, broadcast shortly after Pearl Harbor, contained two dominant themes. The first stressed the power and potentiality of the United Nations, being intended to combat defeatism. The second emphasized the strength of the enemy in an effort to combat complacency and over-confidence. The problem is clear enough. Is it not possible that emphasis on our strength will reinforce the complacency of those who are already complacent? And correlatively, that references to enemy strength will support the defeatism of those who are already defeatist?* To judge from interview materials, this is evidently what happened.

It is no easy task to avoid opposed reactions by different sections of the audience. It is further complicated by shifts and, it would sometimes seem, by mercurial shifts in the "state of the public mind," such that the prevalent outlook is at one time "complacent" and, at another, "acutely pessimistic." Once again, it appears that if "morale propaganda" is to be functionally appropriate to the situation, there must be a continuing intelligence concerning dominant emotional orientations of the population.

A third type of boomerang is perhaps more significant than the others, for it is one which can be largely eliminated on the basis of adequate propaganda analysis. This we may call the *structural boomerang, which results from different themes in the same piece of propaganda working at cross-purposes.* If the propagandist considers *separately* the several themes in his propaganda and ignores their social and psychological interrelations, he may find that his total propaganda document is ineffective in reaching his ends. Structural analysis of the relations between themes is necessary if this is to be avoided.

A hypothetical case, parallel in essentials to instances which have actually emerged in tests, may serve to illustrate the structural boomerang. Several films, produced before American entrance into the war, included two dominant themes, among others. The first of these emphasized the immense cruelty and sadism of the Nazis as well as their threat to our way of life; a theme vividly exemplified by scenes of mistreatment of civilians simply because of their political or religious convictions. In interviews, sequences such as these are found to evoke profoundly aggressive feelings on the part of many in the audience.

But curiously enough, such aggression directed against the Nazis does not necessarily lead a larger proportion of those who have seen these films than of those who have not to express their willingness for this country to enter the conflict. In fact, there may be at times a slight decrease in the numbers of the "film-group" as compared with the "control-group" who wish us to intervene in the war. How does this come about?

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* In fact, there is some experimental evidence, however slight, that persons respond selectively in such manner as to reinforce their current attitudes and sentiments.

On occasion, interview material will show that this apparent absence of effect so far as intervention is concerned derives from the fact that another theme in the film works at cross-purposes. This counteracting theme may stress the skill, experience and enormous size of the Nazi army, exemplifying these by detailed and vivid sequences of Nazi fighting men in action. A theme such as this may serve to elicit fears and anxieties about the prospect of Americans coping with armies as formidable as the Nazi, particularly since we had not yet built up our own forces.

Thus, it may develop that the Nazi-strength theme which elicits fears may counteract the Nazi-cruelty theme which elicits aggression. Aggressive feelings may thus not be translated into a realistic desire to have this nation enter the conflict. Adequate structural analysis of such films would have indicated the likelihood that one theme in the film would inhibit the very effects deriving from another theme in the same film. Consequently, although each theme may be effective, as it were—the one in exciting hostility, the other in acquainting Americans with the might of the enemy—the net result, so far as willingness to have us intervene in the war, may be nil.

This type of case not only illustrates a type of boomerang-response, but also shows how the focused interview enables us to supplement and enrich the value of the traditional controlled experiment, of the type mentioned at the outset of our paper. The controlled experiment consists in having two closely matched groups of subjects, one of which has been exposed to the propaganda, the other of which has not. Certain attitudes and sentiments of the two groups are tested twice: once, before the experimental group has been exposed to the propaganda; again, at some time after it has been exposed. If the groups are indeed properly matched, differences in attitude between the two groups which are found in the second test can be ascribed to the propaganda. But let us suppose that, for some attitudes, there is no perceptible difference, as was the case with our subjects' attitude toward American intervention in the war. The controlled experiment will not tell us why there is no change. *Its results show only the net effect of the propaganda on this attitude and not the more intricate dynamics of re-*

sponse which led to this net effect. But, as we have seen, the failure of the film may be due to the fact that two themes, each of which was effective, produced responses which cancelled each other out. The interview material thus enables us to provide a psychological explanation of responses which may not be registered in the experimental results.

A fourth type of boomerang should be briefly discussed, if only because it is so frequently found in propaganda. *This boomerang results from what we may call,* with due apologies to Whitehead, *the fallacy of misplaced exemplification.* Whenever propaganda deals with matters which are familiar at first-hand to the prospective audience, there is the risk that the particular examples chosen will not be considered representative by some in the audience who consult their own experience. The pamphlet dealing with Negroes and the war which we have previously discussed was largely devoted to the social and economic gains of Negroes under American democracy. This theme was exemplified for the most part by photographs of prominent Negroes, of improved housing conditions and the like. Some 40 per cent of a sample of Negroes discounted the entire pamphlet as "untrue," because of the marked discrepancy between their own experience and observations, on the one hand, and these "examples of progress," on the other.

It should be noted that the truth of the examples does not spare them from producing a boomerang response. The reader consults his own immediate experience and if this does not correspond to the examples contained in the document, he rejects these wholeheartedly. The distrust generated by such apparent discrepancies between "fact" and the "propaganda" tends to be generalized and directed toward the document as a whole.

Moreover, boomerang responses diffuse far beyond the persons who experience them initially. In discussing the document with others, the distrustful reader becomes, as it were, a source of contagious scepticism. He predisposes other potential readers toward the same distrustful attitude. Thus, content-analysis and response-analysis, which eliminate such bases for boomerang responses, serve an important prophylactic function.

Our account has perhaps included enough examples of propaganda analysis to help overcome a perennial difficulty with writers and producers of propaganda. The creative writer can often not accept the notion that what he has conceived as a unique expression of an inspired moment could possibly be improved or even dealt with by what seems to him a rather mechanical testing procedure. But this is all beside the point. It is not assumed that we are getting at the mind of the artificers, the craftsmen, the artists who contrive this propaganda. It is not believed that our prosy analysis recaptures the deft rhetoric and impressive rhythms which enter into its dramatic effectiveness. It is agreed that we cannot readily teach them their craft. Creative ideas, whether expressed in words, sounds or pictures, cannot be manufactured synthetically.[9] But systematic research is needed to see whether propagandists have achieved their aims. Just as researchers cannot write acceptable scripts, so, we are convinced propagandists often cannot gauge the psychological effects of their products without using techniques such as we have described. It might even be conjectured that it is in the nature of his problem that the propagandist is bound to overlook some of the undesired implications of his work.

This may explain the frequency with which our tests uncover inadequacies which, it would seem, should have been anticipated. But, in fact, response-analysis is usually indispensable: it uncovers a host of other inadequacies which we cannot now discuss at any length. This extends to modes of presentation. For example, consider the technique which the radio has adapted from the movies, the quick shift of scenes corresponding to montage in visual presentations. We are confident, on the basis of tests, that this technique in general leads to obscurity for the average radio listener. Continuity is lost. They just don't know what it is all about. They lose interest. In much the same way, historical allusions often fall on deaf ears unless they are carefully explained.

Or consider the question of authenticity in the case of documentary films. Propagandists would probably be surprised to learn how often the audience questions the possibility of having an actual film of Hitler in his mountain retreat, or of the mountainous Goering in a conference room. The propagandist knows that it is a clip from a German film, but the audience does not. Distrust is engendered and spreads. In the same way, we have found numerous errors of judgment in the use of radio narrators or of officials' speeches which outrun the endurance of the audience. But details of this sort would consume the remainder of our available time.

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[9] We should thus agree whole-heartedly with the views of Aldous Huxley on essentially this same issue. " . . . the man of letters does most of his work not by calculation, not by the application of formulas, but by aesthetic intuition. He has something to say, and sets it down in the words which he finds most satisfying aesthetically. After the event comes the critic [read: propaganda analyst], who discovers that he was using a certain kind of literary device, which can be classified in its proper chapter of the cookery-book. The process is largely irreversible. Lacking talent, you cannot, out of the cookery-book, concoct a good work of art." "T. H. Huxley as a man of letters." Huxley Memorial Lecture. 1932, p. 28; also **Remy de Gourmont**, La culture des idées, 1900, p. 51.

We have repeatedly emphasized the need for obtaining detailed evidence of responses to propaganda. As an aid toward this end, we have often used a device called the Program Analyzer. The device, so called because it was first used for radio tests, can also be used for any communication, such as a film, which develops along a time-dimension. The purpose of the Program Analyzer may be briefly explained. Interviews on responses to propaganda must of course be postponed until the film or radio program is over, since we do not wish to interrupt the normal flow of the audience's experience. How, then, can we help the audience to recall their responses to particular aspects of the material? Should the interviewer mention specific scenes or episodes, he would be determining the focus of attention. Moreover, the interviewer's description of the scene would also influence the respondent's account of his experience. The Program Analyzer serves to eliminate these limitations.

While watching a film or listening to a radio program, each subject presses a green button in his right hand whenever he likes what is being presented, and a red button in his left hand when he dislikes it. He does not press either button when he is "indifferent." These responses are recorded on a moving tape which is synchronized with the film or radio program. Thus, members of the audience register their approval or disapproval, *as they respond to the material.* Reasons for and details of these reactions are later determined by the type of focused interview to which we have referred.

Two advantages of this procedure are clear. In the first place, the audience itself selects the sections of the material which are significant enough to be made the object of a detailed interview. Each listener presents, as it were, a general running account of his own reactions by classifying the material into three groups: the items which affect him positively, negatively, or neutrally.

Secondly, the responses recorded on the tape can be cumulated for the audience as a whole to obtain a general "curve of response." This curve lends itself to statistical treatment, enabling us to determine the main sources of favorable and unfavorable response. Above all, it provides, together with prior content-analysis, an extremely useful guide to the focused interview.

TECHNOLOGICAL PROPAGANDA OR THE PROPAGANDA OF FACTS

This discussion has perhaps served its major purpose. It may have given you some conception of procedures used in the psychological analysis of propaganda. Now let us turn to some general conclusions which we have reached in the course of our work.

One of the most conspicuous responses which we observe in our tests is the pervasive distrust of propaganda exhibited by many people. Propaganditis has reached epidemic proportions. Any statement of values is likely to be tagged as "mere propaganda" and at once discounted. Direct expressions of sentiment are suspect. Comments such as the following are typical of the ubiquitous man in the street when he believes that others seek to sway him:

> I just think it's too sappy to put over on an adult mind. To me it gave the opposite kind of a reaction than it was supposed to give me. I suppose they wanted to make you feel full of patriotism, but I think it gave me the opposite reaction.
>
> And then at the end—whistling the Star Spangled Banner. Everybody believes in the flag, but they don't like it waved in front of their faces.

This distrust of sentiment will not surprise you. There appears to have been relatively little fanfare during this war. As the psychoanalyst, Ernst Kris, has put it, referring to our enemies as well as ourselves, "men went to war in sadness and in silence."[10] Or, in the words of a subject in one of our tests:

> In this present situation, we haven't seen the boys marching as we did in 1917. We haven't got the feeling of the situation.

What implications does this lack of collective outbursts of enthusiasm have for the propagandist who seeks to rally all support to the war effort?

Our observations suggest that such distrust is levelled primarily against propaganda which obviously seeks to "sway" or "stir" people by general appeals to sentiment. Efforts to excite diffuse emotions are discounted. But this is only a partial scepticism. The same audiences which set up defenses against fervent appeals to patriotic sentiments show a readiness to accept the implications of another type of propaganda, which we may tentatively call *technological propaganda* or *the propaganda of facts.*

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[10] It is interesting that, basing his discussion on quite different propaganda materials, Ernst Kris has independently come to much the same conclusions. See his instructive paper: Some problems of war propaganda. The Psychoanalytic Quarterly **12**: 381-399. 1943.

Again let us begin with observations made in the course of our own studies. We observed at once a central interest in *detailed circumstantial facts.* Facts are in the saddle. The following comment by a subject in one of our tests reflects this attitude:

> A great many people [*sic*] don't like that rah-rah sort of patriotism that stirs you up. I [*sic*] like factual things.

This desire for specific, almost technological information, sometimes takes on naive forms, as can be seen from the following remark on a documentary film which stressed the strength of the Nazis:

> I was really surprised. I mean I don't believe everything I have read in the papers. But what you actually see with your own eyes and is authentic, you have to believe.

One of the most effective scenes in the aforementioned radio morale program described in great detail how the speed of a convoy is not necessarily determined by the speed of the slowest boat. Wrapped in this layer of technical information was an effective implication that men in the merchant marine willingly sacrifice themselves for the common good. The morale contained in the facts—"surely my sacrifices do not match theirs"—could be accepted by those who would reject a direct appeal of the same type. Films showing battle scenes or bombings prove effective if they focus on the details of the operations rather than stressing the direct propaganda "message" for the audience. *The facts, not the propagandist, speak.*

We may now ask: why this prevalent interest in "facts"? What are the functions of this interest? *The concrete incident, rich in circumstantial detail, serves as a prototype or model which helps orient people toward a part of the world in which they live. It has orientation-value.* For large sections of the population, the historical events which they experience are wholly bewildering. Nations which are enemies one day are allies the next. The future seems dark with despair or bright with promise. Many have not the time or capacity to understand the trends and the forces behind them, yet they sense how closely these are bound up with their lives. All this accentuates a powerful need for orientation. Concrete facts take on the role of models in terms of which more complicated events can be explained and understood.

Illustrations of this are numerous. Thus, one episode in a radio morale program made a notable impression on the audience: during the last war, Franklin Delano Roosevelt, then Assistant Secretary of the

Navy, accompanied a submarine crew on a trial run, immediately after a series of submarine disasters. This proved far more satisfying and effective than to be told directly of the courage and past experience of our President. It had an *integrating,* explanatory function.

> He showed he wasn't a coward; that if the men were willing to go down, he was willing to go; and he's the best man to be president because he's been through the thing himself, and because of the things he's done.

So, too, when films indicate specifically the virtual absence of armored divisions in England after Dunkirk, this type of fact will effectively integrate a variety of discrete points. It will be mentioned repeatedly in interviews. It helps to crystallize, so to speak, the ingenuity and courage of the British in the face of such odds. It proves effective where direct evaluations of the British would evoke scepticism and doubt. *Facts which integrate and "explain" a general course of events comprise one important component of the propaganda of facts.*

We can make another general observation about the propaganda of facts. We have observed that a certain type of fact which contains the desired propaganda implications appears to be most effective. This is the *"startling fact,"* of the type exploited by "believe it or not" columns and by quiz programs. This is effective for at least three reasons. In the first place, it has great *attention-value.* The startling fact stands out as a "figure" against the "ground." Secondly, such tidbits of information have *diffusion-value.* They readily become part of the currency of conversation and small-talk ("Did you know that . . ."). The propagandistic implications of these are thus often transmitted by word of mouth. Finally, these integrating startling facts have *confidence-value.* They are "cold," as idiom so aptly puts it. They are not likely to elicit the distrust which is so widely latent in the population.

The propaganda of facts has yet another characteristic which marks it off from propaganda which seeks to persuade by clarion calls and direct exhortation. The propaganda of facts does not seek so much to tell people where to go, but rather shows them the path they should choose to get there. It preserves the individual's sense of autonomy. *He* makes the decision. The decision is voluntary, not coerced. It is by indirection, not by prescription, that the propaganda of facts operates. It has *guidance-value.* The cumulative force of facts carries its own momentum, so to speak. It is virtually a syllogism with an implicit conclusion—a conclusion to be drawn by the audience, not by the propagandist. To take a case in point, a pamphlet was recently issued by a war agency, directed to the families of men in the armed service, for the purpose of persuading them not to repeat the contents of letters received from abroad. Little emphasis was placed on the theme that careless words cost lives and ships. Instead, the bulk of the pamphlet was devoted to a detailed description of the methods used by the enemy to construct their total information from bits and patches gathered by agents on different occasions and in different places. Tests showed that the pamphlet succeeded in driving the story home, by permitting the reader to draw the inevitable conclusions from this circumstantial array of facts. The voluntary drawing of conclusions has little likelihood of the aftermath of disillusionment which so often follows upon the propaganda of exhortation. The hammerlike blows of frenzied oratory may produce present acquiescence and later recriminations; autonomous decisions under the cumulative pressure of facts do not exact this price.

Interesting enough, it appears that our enemies have also discovered the power of technological propaganda. This type of propaganda, as any other tool, may be abused as well as used. The pseudo-facts may supplant the fact. Several observers have commented on the Nazi "stage-managing" of reality. It is reported, for example, that prior to the invasion of Belgium, a German officer made an apparently forced landing in Belgium. On his person were found plans for an invasion quite unlike that actually intended. Or again, there is the case of the first night bombing of Berlin. It is said that the Nazis planted reports of great destruction in Berlin in Swiss and Swedish newspapers, accrediting them to the English. These accounts were rebroadcast over the German domestic radio and the local population was invited to look at the actual damage and see for themselves that the reports were untrue. In this way, probably, many people could not escape the conclusion that the British had lied. The effect of this type of self-indoctrination was probably considerably greater than if the German radio had directly denounced the veracity of the British.

In passing, it might be remarked that the logic of the propaganda of facts is not far removed from the logic of progressive education. It is typical in progressive schools that the teacher does not dictate what the children are to do and believe but rather creates situations which lead them to decide for themselves the conduct and beliefs which the teacher considers appropriate.

Your own experience will demonstrate that the propaganda of facts is not a "new" conception. We are concerned only with formulating this idea in terms which may be of some value in planning morale programs. Widespread distrust and scepticism pushed to the extreme of cynicism are corrosive forces. But, since they are here, they must be considered. If propaganda is restricted wholly to exhortation, it runs the risk of intensifying distrust. The propaganda of facts can be utilized to supplant cynicism with common understandings.

Nor do we suggest that exhortations are wholly a thing of the past. Common values and common attitudes still need to be established among a considerable part of the population if propaganda is to prove effective. But our observations may be useful to those of us who are concerned with a constructive post-war era. We should not wait until post-war problems press in upon us before we recognize that a re-integration of societies must, to some extent, draw upon the instrument of propaganda.

And, finally, we should not exaggerate the role of propaganda. In the long run, no propaganda can prevail if it runs counter to events and forces underlying these events as the fascists have begun to discover. Propaganda is no substitute for social policy and social action, but it can serve to root both policy and action in the understandings of the people.

NEW MEMBERS

Elected November 18, 1943

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Auerbach, Johann G., M.D., Psychiatry, Psychoanalysis, Biology, Practicing Psychoanalyst, New York, N. Y.

Eaton, Theodore H., Jr., Ph.D., Zoology and Paleontology, Assistant Professor, Biology, University of Buffalo, Buffalo, N. Y.

Hocket, Robert Casad, Ph.D., Chemistry, Scientific Director of the Sugar Research Foundation, New York, N. Y.

Mancini, Armando F., M.S., Bacteriology, Professor of Bacteriology and Registrar, American Academy of Embalming and Mortuary Research, New York, N. Y.

Maslow, A. H., Ph.D., Psychology, Instructor, Department of Psychology, Brooklyn College, Brooklyn, N. Y.

Mason, Herman C., Ph.D., Bacteriology and Immunology, Associate Professor, University of North Carolina, Medical College, Chapel Hill, N. C.

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Shuttleworth, Frank K., Ph.D., Psychology, Professor, Columbia University, New York, N. Y.

TRANSACTIONS
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THE NEW YORK ACADEMY OF SCIENCES

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SECTION OF GEOLOGY AND MINERALOGY[1]

DECEMBER 6, 1943

PROFESSOR GLENN L. JEPSEN, Curator of Vertebrate Paleontology, Princeton University, Princeton, N. J.: *Phylogenetic Trees.*[2]

Some of the most difficult and elusive problems of phylogeny, the evolutionary history of organisms, are now being reattacked by a combination of techniques and information from paleontology and genetics. Neither of these disciplines, nor any one of the many other biological and geological sciences such as comparative anatomy, embryology, stratigraphy, and ecology, is wholly adequate for rational explanations of the complexities of evolution. Each, however, contributes essential data for a synthetic approach to fundamental questions about the rates and processes of evolution which, itself, has been called the "central fact" of biology and is the principal guide in geological chronology and the correlation of strata.

With the development of new opinions about the methods and speeds of evolution, the patterns of family trees which are popular as abbreviated and graphic interpretations of phylogenies have been modified. A quarter of a century ago the common or garden variety of phylogenetic tree was the familiar diagram which looked like an oak or a maple with a central trunk and spreading branches. Only the occa-

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[1] No meetings were held in December by the Sections of Biology and Anthropology.
[2] At the time of its presentation, this paper was titled "Geology, Genes, and Genealogy."

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sional dashed line which, as the label said, indicated inference (and thus suggested that the solid lines were facts) prevented these trees from looking like rather poor botanic sketches. The growth process, unlike anything in nature, consisted of a series of grafts. If a limb withered in one position it was hacked off and attached somewhere else. Or, a tree could begin as a clone or cutting suspended in mid-air and develop in any one direction or in many.

Most paleontologists had their own nurseries of seedlings in their laboratories and their published groves of adult trees in their bibliographies. Nearly every publication date was Arbor Day. Naturalists almost came to regard these paper and ink trees as true works of nature, and became ardent conservationists with the unstated but rigidly practiced motto of "Woodman spare that tree!"

For the welfare of the phylogenetic woods as a whole there were probably too many of these trees and their patterns were too standardized, but occasional comments that the stand of good timber could be improved by logging through the whole forest were met by the suggestion that there were better places to use well-ground axes. In one or two notorious instances the trees grew so rapidly and then decayed at such a rate that the authors who had fabricated them found themselves out on limbs with no visible means of support. The resultant crashes served as warnings that genealogical trees for fossils must be strong enough to support the opinions they are devised to symbolize and that large branches shouldn't be inferred too readily from small morphologic evidences. .

At this same time, in the early thirties, other factors contributed to an increasing scepticism about the accuracy of some of the pedigree diagrams. When geneticists suggested that these sketches had too few genes and chromosomes in their growth formulae, paleontologists, as alert forest wardens, immediately recognized this possibility and began to suspect that geneticists themselves would sooner or later learn to ascend phylogenies, microscope in hand, or even to grow them if rearrangements and mutations of genes were the seeds of all evolution. This belief, that geneticists by their ingenious manipulations of *Drosophila* were developing precise new techniques which would provide all of the answers for all of the problems of evolutionary sequences, had a curiously intimidating effect upon paleontologists. The "chemical accident" concept of alterations in the genal complex seemed to mean that structural changes were entirely fortuituous and non-adaptive in origin, and this appeared to be a blight upon the family trees.

What the paleontologists didn't know was that geneticists among themselves had many differences of opinion about the meaning of their own work and were, in general, too occupied with chromosome forms and numbers and gyrations in mitosis and meiosis to be very concerned about extrapolating these data into studies of evolution. Paleontologists tried, without much success, to interpret the significance of the short time of genetics experiments and the minuteness of viable mutations in terms of the millions of years and the profound morphological changes of phylogeny.

As the literature of genetics rapidly grew in volume, it also became increasingly esoteric. In the lack of a summary statement of its meaning by the geneticists themselves, paleontologists tried to translate it, learned some of the jargon of Mendelism and cautiously attempted to apply the words and concepts of paleontology.

But the new patois didn't encourage the growth of family trees. Even some of the old established groves looked a bit withered. Perhaps they needed trimming. Paleontologists, in their anxiety, founded the sceptical school of phylogenetic tree surgery, sharpened their erasers, and went to work. They did a thorough job. When they had finished pruning they reassembled the fagots into two general kinds of sketches. One kept the old oak pattern but consisted entirely of dashed lines and the other looked like the candlewood ocotillo with its twigs ascending almost parallel from the ground. No one cared or dared to draw a solid line from any one fossil group to any other. Paleontologists who worked with abundant material, like that provided by the rodents, composed the chant "Parallelism, parallelism, and still more parallelism."

When geneticists saw the new forest of family stalks, they concluded that paleontologists had revised the former pattern from specifications which were dictated entirely by the fossils themselves. This of course was not true, and, although the former growth had been too lush, it appears from the perspective of the present time that the conversion from tree to pole may have been too complete, that some sound trees were needlessly destroyed in anticipation of diseases which never developed. Paleontologists are discovering now that geneticists neither had a magic tree growth formula composed of cell nuclei nor had been particularly concerned about compounding such an elixir. Unfortunately, some paleontologists have concluded that genetics has little or nothing to offer to paleontology, but many of the clan are pooling their techniques and data with those of the geneticists in an attempt to make some family trees which will satisfy the facts and the theories of phylogeny. Successful results will have far reaching effects in biology and geology.

The participants of this common effort are finding that their motives and methods have some close similarities and some considerable differences, and that their general conclusions are in harmony. Geneticists can advance these studies by transferring more of their attention from gene mutations to structure and paleontologists can offer case histories and time to test some of the theories of genetics. Already, many notions about orthogenesis and irreversibility and other concepts are being modified by the confluence of evidence from the two sources. We are beginning to see why some of our former discussions about the relationships of environment and structure and function were futile, because these factors now appear to be inseparably interlocked in natural totality.

Genetics is essentially an experimental science, paleontology is historical. They join in trying to "predict" what has already happened. There are many difficulties, in addition to the differences in jargon, which work against complete mutual understanding between specialists in the two disciplines, but these do not appear insurmountable. For a while, there was a tendency to assume that genetics, or at least that part of the subject which can be applied to paleontology, was far simpler than it is, that, if a mechanism of inheritance were discovered in one group of organisms, it could be applied to all plants and animals whether they consist of one or of many cells.

Paleontologists have been unable to pace the literature of genetics from the time it raced beyond primary Mendelian inheritance and the simple dominant-recessive allel relationship. They have been puzzled about the degree to which laboratory materials in controlled and artificially stimulated short-term experiments resemble natural situations. Geneticists, too, of course, have been interested in this problem and have hunted and discovered instances where mutation rates and kinds in nature are quite similar to those which occur in bottle cultures.

Many data of paleontology can become increasingly significant and less receptive to geologic abuse as their interpretation is aided by genetics. Attempts to correlate the temporal and geographic and somatic features of fossils with genetic principles are highly involved enterprises however, with no obvious routes to success. It is desirable, in studying fossils, to be able to discriminate between structures which may be treated as inheritance factors or units (regardless of whether they were actually controlled by one or many or no single genes) and those which were individual developmental modifications.

Direct tests for distinguishing genotypic from phenotypic variations in living forms are largely experimental, but there are numerous possibilities of interpreting the structures of fossils as exemplifying one or the other. The hypostyle on the teeth of early Oligocene horses once seemed to be an individual phenotypic variation whose presence or absence was controlled by ontogenetic accidents. Now it seems possible to interpret the cusp as a relatively simple gene mutation, probably dominant, and to trace its diffusion into and through populations of *Mesohippus* and beyond, for forty million years to the present time.

Serious errors in stratigraphic correlations can result, if the hypostyle is regarded as an important or determining criterion for the separation of species or genera, but, if it is recognized as the manifestation of a drifting gene, the possibilities of misusing it in geological work are reduced. This appears to be a double-barreled paradox for, while we decrease the significance of the hypostyle as a morphologic and systematic item, we simultaneously increase its geologic and genetic values and its resistance to taxonomic treatment.

Geneticists, like most neontologists, are handicapped in attempting to help the paleontologist, because they lack experience with the techniques of establishing concepts of absolute and relative geological time, and with the colossal number of fossils and of kinds of fossils that have been collected and studied. Partly for these reasons neontologists fail to understand the concern of paleontologists about the fact that our system of nomenclature names stages of a process, elements of a continuum, and yet has no simple way of indicating their temporal or structural positions. It is unfortunate in taxonomy that categorical names do not evolve, although the nominees continuously do so. This inadequacy of our language symbolism is a serious but not an unusual situation. A developing science freqently inherits its first vocabular wardrobe from older relatives and we patch and let hems out until too many of the seams split before we make clothes that fit.

Not only tooth cusps but other structures of fossil horses have been and can be still further utilized for projection of genetic tests, because horses are among the most numerous of fossil vertebrates and their evolutionary history is well known for most of the Tertiary. Using low figures for numbers of individuals and generations, it has been calculated that if all of the horses that have lived from *Eohippus* (or, *Hyracotherium*) to *Equus* were to form a parade seventy-five miles wide and to walk steadily, as close together as possible, at six miles per hour, they would require ninety years to pass the reviewing stand where the taxonomists would be going mad trying to establish dividing lines, where none existed in the equine spectrum, between species or between genera.

A pile of photographs, one of each horse, on paper of the thickness on which this is printed, would be about ten million miles high. No one knows how many thousands of fossilized specimens have reached museums, but certainly only one horse carcass in several million has survived the hazards of burial, fossilization, solution in the rock, removal by erosion, and the chances of discovery. For other groups, such as the early Tertiary primates, the museum expectation is probably much lower, perhaps on the order of one chance in ten to the tenth or fifteenth power.

Attempts to correlate rock thicknesses with degree of morphologic differences have sometimes led to irresponsible statements that one organism could not have been descended from some particular earlier form because the time interval between them was too short for such changes to occur. This interpretation is of course legitimate for some situations, and the principle involved in it has other important applications, but there are many cases where conclusions based upon it will be dubious until we know more than we do now about rates of evolution.

Concepts about "index fossils" can be clarified and their utility in correlation of strata improved by more attention to principles of evolution and genetics. We have all been told that the best index fossils have short time ranges and wide space distribution. "Short time range" frequently means that a certain kind of fossil has been found only in narrowly limited zones. A primary knowledge of genetics and paleontology might have absorbed or prevented some of the shocks that geologists have had upon learning that the geologic position of certain forms was controlled by ecologic or facies factors and had unpredictable temporal significance. Correlating strata by the presence in them of organisms of unknown past and future is a dangerous and treacherous practice.

With the techniques and materials now available in genetics and paleontology, we can make further progress in resolving some of the intriguing puzzles of phylogeny. Although, at first thought, there would appear to be nothing more difficult than an estimate of the genetic make-up and environments of a fossil which has no genes and whose milieu is concealed by rock and time, nevertheless, the tantalizing residues suggest many research topics. It might be possible even to detect a gene creeping across a Paleozoic land or water bridge among a population which was itself sedentary; or to observe the mating of a pair of alleles in the Mesozoic; or to follow the ascension of a mutation through several thousand feet of Tertiary sediments; or to infer that some Horatian gene kept an ancient bridge against certain groups, but not others. Whatever a gene is, it may be one of the most durable matters in nature.

To some of the following questions which are pertinent to the specifications for family trees, we already have incomplete answers from genetics and paleontology as well as from other sources, but further documentation and integration of the topics is desirable.

(1) Is there a general or normal cruising speed of evolution? Would the average graph of morphological change in a group or phylum (in the linear sense), plotted against time, be a straight line?

(2) To what degree does the slope of such a curve depend upon population structure and upon distributional factors? Do small numbers of inhabitants of limited insular areas consistently transmute more rapidly than large continental groups?

(3) Are rate fluctuations within an evolving phylum concomitant or correlative with regional geologic or worldwide cosmic events, or with accelerations and decelerations of other groups?

(4) Are there times of extreme variability near the origin or the decline of a group?

(5) Is there any correlation between variability of organism and geologic span of its existence?

(6) Does a group dichotomize by the assumption of differential rates of evolution by population fractions? If so, what is the nature and duration of the anastomosis or inosculation while discontinuity is being established? What genctic and nongenetic factors can cause discontinuity?

(7) For how long and to what extent is reversibility an actual pattern?

(8) If homologous mutation is an explanation of morphologic

parallelism, what causes the phenomenon? To what extent is gene mutation non-random?

(9) What are the genetic mechanisms which have produced the numerous instances of convergence where groups of organisms develop genetic diversity and morphologic similarity? Can we establish any principles with high predictive value from these examples?

(10) Is linkage a common factor in the cases which have been called overspecialization and extinction and orthogenesis? How many historical examples do we have of differential rates of mutation of contrasting morphological features which are genetically linked? Is it possible for one quality or structure to mutate toward optimum adaptive development through orthoselection and at the same time induce or control other mutations which are progressively inadaptive or lethal? Are the antlers of the Irish elk, a sex-limited structure, an example of some such type of unfavorable carrier linkage, a kind of genetic suicide pact?

One additional question that has caused much debate between and among geneticists and paleontologists is "What causes the dearth of connecting or integraded forms between the major kinds of organisms?" The answer, apparently, is that the question is premised partly upon an overestimate of the number of gaps in the record, and that there may be several reasons for our failure to find and identify more intergrades than we actually do. Some geneticists and paleontologists have exaggerated the missing-link concept and have proposed enormous systematic mutations (*macrogenesis*) as the explanation. Statements like the half-serious quip that "The first bird hatched from a reptile's egg" emphasize the suggestion that the record gaps occur at the most critical points in phylogeny, between the divergent dashed lines of the family tree.

There are, in general, three kinds of intergrades: (1) a section of a branch, (2) a Y or a fork where a branch divided into two of about equal size, and (3) the origin of a minor limb from the main trunk. Paleontology can sometimes, but not always, distinguish between the latter two because, as with natural trees, a forked seedling may develop into a twin tree with trunks of about equal size, or either branch may become the larger one, or their size relationship to each other may be reversed one or many times.

All members save the terminal ones of any single evolutionary continuum are intergrades of the first kind and these are, of course, well known and recognized, but on the diagrams little interdash spaces are frequently placed between them, to receive subsequently discovered dashes, or just in the name of caution.

The second sort of intergrade is in the region of a dichotomy. Vertebrate paleontology has apparent examples of organisms at or close to the point of branching of the Y pattern for every two diverged classes from fish to mammal and to bird, between crossopt and amphibian, between amphibian and reptile, between reptile and mammal, and between reptile and bird. These forms have the great merit of occurring at about the proper times, so that they are not as anachronistic as many "structural ancestors" (those morphological treasures that lived too late).

Many Y cases could be cited at all categorical ranks. These are not nearly as rare as they are held to be in popular scientific opinion. Indeed, in numerous instances where strong evidence suggests that two groups dichotomized from one, we have more than one candidate for the ancestral position and can't get a quorum to legalize the election. This admission is probably revealing, for, to change the metaphor, when we tug at the cloak of knowledge to cover one area of ignorance, we expose another to the chilly drafts of inquiry.

After the two "good" genera of saber-toothed cats, *Hoplophoneus* and *Eusmilus,* were described and named, a third form, a new species, was recovered from older rocks. It constitutes an admirable ancestor for both *Hoplophoneus* and *Eusmilus.* Clear as the concept may be that this is a case of the origin of two species from one, taxonomy makes the divergence appear to be generic in one of the directions. Morphologically, the new type resembles members of species of the other two genera, about equally, and in the absence of either genus would be placed in a new species of the other. To manufacture another genus would be incompatible with the calibre of generic grouping among the felids. This dilemma ended, but without solution, when the new species was allocated to one of the already established genera, *Hoplophoneus.* In the original description, this puzzling situation was noted, with the comment that the disjunction and recombination of the roots of the two generic names *Hoplophoneus* and *Eusmilus* would have provided a word, "hoplosmilus," which might suggest the morphological relationships of the case. A foreign author facetiously replied that the reciprocal root-combination pleased him more, being "euphoneus."

A yet-undescribed bat from the early Eocene, possibly the oldest known representative of the order, illustrates a Y case involving suborders. The two suborders of modern bats can be characterized by two very obvious structures. Megachiroptera, the large frugivorous tropical forms, have a claw on the index finger, and have simple teeth, whereas the Microchiroptera lack claws but have teeth of a complex form. The new fossil shows the claw of the Megachiroptera and the tooth-form of the Microchiroptera and, so far as the evidence is known at present, could have been ancestral to either or both of the living suborders. It is to be treated as a member of a new suborder.

There are also numerous examples of the third kind of intergrade:—at the place where the twig issues from the main trunk, the small population evolves divergently from the common herd. Two examples of highly specialized mammals may be cited. *Galeopithecus,* the doubly misnamed "flying lemur," is the sole living form in the order Dermoptera, and *Orycteropus,* the "earth pig," has a similar distinction in the order Tubulidentata. The former is insularly distributed in the Philippine Island area, and the latter lives in Africa. Both may be regarded as examples of small populations, although individuals are locally numerous, and this situation may have endured since the early Tertiary.

The incisor teeth of the flying lemur are nature's patent on the comb, an extraordinary dental structure the function of which is still debated by naturalists. *Plagiomene,* from the Wasatchian Eocene, had an incipient development toward this specialty and, in other almost equally remarkable ways, forms an intergrade about midway from a generalized insectivore to *Galeopithecus.*

The strong legs and peculiar pulpless teeth of *Orycterops* provide the means for digging and eating termites, almost its only food. Instead of lacking primeval relatives which foreshadowed its peculiarities, this genus has been provided with two claimants by paleontologists. Both are of early Tertiary age, *Ectoconus* from the Paleocene, and *Tubulodon,* the rare Wind River Eocene form. This case will remain a taxonomic impasse until further evidence is discovered, but it illustrates how an intergrade may be nominally missing, yet possibly present. Taxonomists cannot decide which is the more likely ancestor and hence place neither in the Tubulidentata, and the order thus appears on the records as having no ancient forebears.

These instances illustrate some of the ways that no-longer-missing links may be concealed in the literature and camouflaged by systematics, but the principle evolutionary speculation of macrogenesis, that the major genetic and morphologic advances occurred in single large steps, has such great significance to paleontology and stratigraphy that it should be very carefully considered. If catastrophic structural changes in linear phyla were a common or actual mode of evolution, we could not expect them to occur in all lines simultaneously, and yet, in many instances in the stratigraphic record, this might appear to be and has been cited as the case. Locally, the organisms above a certain horizon in a continuous rock sequence may differ considerably from all related forms found at adjacent lower levels, but, in every case which can be illustrated by reasonably complete evidence, the explanation is hiatus, non-deposition, a gap in the temporal and sedimentary record as well as in the phylogenetic sequence. The missing records have so frequently turned up elsewhere, only slightly worse for wear, that we can use the degree of morphologic change during a hiatus as a rough guide to estimating its span. Actual fluctuations in evolutionary rates appear to be local in terms of place, time, and biologic group.

The evidence for *microgenesis,* or comparatively slow evolution by minute changes, increments and decrements, seems to be good. Many genetics experiments indicate that the genetic complex of animals tolerates only mutations of relatively small morphologic expression, that the lethal threshold is low. Paleontologic samples of many phyla show an inverse relationship between the completeness of the record and the morphologic difference from one group to the next. However, even if microgenesis is as general a pattern of evolution as it appears to be, there are still many factors which might prevent our discovery or recognition of intergrades, particularly of those in the most critical positions in phylogeny.

These factors, some of which have little or no evidence to support them, may be divided into two assumptions: (1) that no record survives, and (2) that the fossils were preserved but are rarely found. Under the first might be listed the suggestions: (a) that the tropics, where even resistant hard parts quickly decay or are otherwise rapidly destroyed, were the "cradle of evolution"; (b) that geocratic times, when abnormally large areas were land, or diastrophic periods, when little or no sediment was deposited (and hence opportunities for preservation were at a minimum), are to be correlated with maximum evolution rate; (c) that the fossils were removed by solution after they had been buried; and (d) that they have been destroyed by erosion during and following disastrophism. It is difficult to understand how the latter two factors could act selectively upon most intergrades, but a number of rationales, which will not be repeated here, have been advanced.

There are several explanations for the second assumption; namely, that although fossils of intergrades were preserved, they do not occur with average frequency and hence have fewer chances than other forms of being discovered. Some of these postulates about intergrades are (a) that they lived in areas which have not been thoroughly explored, such as tropical or insular regions, (b) that they existed in relatively small communities, and (c) that, although they evolved by microgenetic changes which were qualitatively and quantitatively average, the rate at which these mutations occurred or achieved morphologic expression was accelerated.

These two latter suggestions are worth particular consideration, for it is possible to interpret much evidence from genetics and neontology and paleontology as supporting them. Paleontology contributes the fact that many apparent Y intergrades are represented by only one fossil specimen or by a few. This suggests that these organisms lived in small communities or were thinly distributed over wider areas. Genetics not only supplies mechanisms that cause somatic innovations, but also provides theoretical means of increasing mutation rates among evolutionary novelties. The effects of rate genes and the release of accumulations of clandestine or cryptic mutations to viable morphologic expression may be accelerated when organisms are out of adjustment with their environment. There are also explanations from genetics and ecology of ways that this imbalance in the sphere of organisms and their surroundings may be accentuated and then gradually ameliorated.

These and other clues to phylogeny and to the shapes of family trees can be pursued by combining the information from genetics and from paleontology. As Pope expressed it,

"T'is education forms the common mind:
Just as the twig is bent the tree's inclined."

SECTION OF PSYCHOLOGY

December 20, 1943

Doctor Margaret Mead, Associate Curator of Anthropology, The American Museum of Natural History, New York, N. Y.: *Cultural Approach to Personality.* Anthropological Comment on the Frame of Reference of Andras Angyal. (Doctor Meal served as substitute speaker for Mr. Geoffrey Gorer, whose absence was necessitated by war service.)

This paper is presented on the assumption that it is one of a series of discussions based on Dr. Angyal's frame of reference, as outlined by him in the opening paper,[1] as a résumé of the thesis developed at greater length in his book entitled, "Foundations for a Science of Personality."[2] In this discussion, I shall limit myself to Chapters I to VI of that work, giving special attention (1) to Dr. Angyal's general frame of reference and its usefulness for providing a meeting place for social anthropologists and other students of personality and (2) especially the specific treatment of culture elaborated in Chapter VI.

Dr. Angyal's use of the term "biosphere," and his insistence upon the usefulness of not singling out organism from environment, but upon regarding the organism and its biologically relevant environment as a whole, is completely congruent with anthropological method, noting that the latter can then appropriately be analyzed either into features, abstractions, or holistic units, "according to its structural articulation." The term, "a culture" (as compared with the term "culture," as used by anthropologists), involves the use of these two sorts of units. Therefore, such abstractions as "ethos" and "eidos" are the types of deutero-learning, as used by Gregory Bateson,[3] the type of abstractions which may be based upon Erik Homburger's graphic presentation of types of modal behavior,[4] or the use which could be made of L. K. Frank's conception of time perspective.[5] In all of these cases, a series of "cultures" may be analyzed into an abstraction which may be conveniently applied to anyone of them. When, however, a culture is regarded as an unique whole, as is possible with isolated and homogeneous primitive societies, then it may be analyzed into units which are primarily relevant to that particular whole, and are not cross-cultural abstractions, but specific cultural holistic units. This approach is best represented by the work of Ruth Benedict,[6] and some of Dorothy Lee's papers.[7] My own work and that of Mr. Gorer,[8] who was to have presented this paper tonight, falls somewhat between the two approaches, using cross-cultural abstractions based upon the development process, but also invoking a number of holistic units specifically referred to the particular culture under description, or, in my own case, to the culture area under discussion.

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[1] Angyal, Andras. Basic sources of humean motivation. Trans. N. Y. Acad. of Sci. Ser 2, 6 (1): 5–13. 1943.

[2] Angyal, Andras. "Foundations for a science of personality." Commonwealth Fund. New York. 1941.

[3] Bateson, Gregory. "Naven." The University Press. Cambridge, England. 286 pp. 1936.

Bateson, Gregory. "Social planning and the concept of 'deutero-learning.'" Science, Philosophy and Religion. Second Symposium. Pp. 81–97. New York. 1942.

Bateson, Gregory. "Some systematic approaches to the study of culture and personality." Character and Personality. 6: (1). 1942.

[4] Homburger, Erik. (Erikson, E. Homburger.) Configurations in play-clinical notes. Psychoanal. Quart. 6: 139–214. 1937.

[5] Frank, L. K. Time perspectives. Jour. of Soc. Phil. 293–312. 1939.

As anthropologists have historically dealt with a group of people sharing a common culture, living in a given environment, they have also traditionally dealt with a biosphere that is included within their frame of reference, composed of both the biologically relevant environment and the organisms. "Tribe A uses neck rests" is a statement that has complete biospheric reference. It may be resolved into the following statements among others: "Individuals born into Tribe A will include, in their symbolic system, the idea neck rest. So, all objects, recognizable as neck rests, will be biologically relevant to them. Actual material neck rests will exist. The neck rests of other tribes, if encountered, will be perceived as neck rests and not merely as curiosities. Neck rests that do exist within the A biosphere will have a traditional form, and the members of tribe A will have habituated methods of making, or acquiring, neck rests of the traditional form. All individuals of normal intelligence within the A culture will react in terms of the existence of neck rests, whether they do or do not use neck rests, and whether they do or do not alter the traditional form of the A neck rest." Re-examination of this series of statements will demonstrate, I think, that any formal statement about a culture includes the idea that organisms who have been so moulded by their experience in a culture that certain types of response to objects, which are regularly within their environment, will themselves be regular. It is important to emphasize here that the social anthropologist deals with regularities, not with uniformities.[9] He is not merely concerned with the number of people who do or do not use neck rests, but with the fact that in a neck-rest using culture, both use and non-use will include neck-rest reference. The extent to which a given number or proportion of the individuals who share that culture at a given time do use neck rests will be one of the holistic comments which can be made upon that culture, but would have to be reduced to a more abstract form, for example: "degree of actual participation of every individual in the culture in a common form of domestic furniture," to be discussed cross-culturally. Confusion between statistical statements of uniformity and statements of regularity are responsible for some of the current misunderstandings of the relationship between the study of culture and the problem of sampling. The fact that every individual in a culture is himself a representative of the regularities of the culture, although he may display a minimum of the usual overt behavior commonly described as characteristic of the culture, is usually omitted. In a crude paraphrase, when other students of personality attempt to include the concept of culture within their frame of reference, they regard using neck rests as culturally determined, but the individual who refuses to use a neck rest would be classified as behaving, in a personal or individual manner, as if he were in some way distinct from the culture. However, the individual who refuses to use a neck rest, and the individual who insists that a neck rest is really a weapon for hitting his mother-in-law, would also be behaving in cultural terms, not *out* of them, for the original inclusion of neck rest within the biosphere would be part of themselves also.

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[6] Benedict, Ruth. "Patterns of culture." Houghton Mifflin & Co. New York. 1934.

[7] Lee, Dorothy. A primitive system of values. Jour. Phil. of Science. 7: 355–378. 1940.

[8] Gorer, Geoffrey. "Himalayan village." Michael Joseph, Ltd. London. 510 pp. 1938.
Gorer, Geoffrey. Themes in Japanese culture. Trans. N. Y. Acad. Sci. Ser. 2, 5 (5): 106–124. 1943.

This confusion between cultural experience and personal experience, in which the personal experience of life is seen, not as something wholly within the terms of the culture, but in some way different and beside it, recurs in Dr. Angyal's discussion. He speaks of the content of an object as being "in part culturally defined and in part built up under the influence of personal experience" (p. 162). This name confusion is evident when he speaks of the channels of dynamic expression as "built up as a result of personal experience and under the *influence* of socio-cultural *influences*" (italics mine). Such statements suggest that an individual has, in the course of his life, cultural experience,—he sleeps on a neck rest; and personal experience,—he sleeps on a particu-

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[9] Mead, Margaret. "Educative effects of social environment as disclosed by studies of primitive societies." Environment and Education: a Symposium. The University of Chicago. No. 54: 48–61. March, 1942.

lar neck rest which has a serpent on it, and has decided he didn't like neck rests. The same lack of systematic inclusion of the concept of culture is manifest in the statement that an individual has to adjust to a culture, that it is "a complex of environmental factors to which one has to adjust; and second, it is assimilated by the individual and becomes an important part of the personality" (p. 206). The first statement postulates culture as something outside the organism; the second as something inside. But, from the anthropological point of view, the individual organism develops *within* the culture from the moment of birth, and, in all probability (although the material to document this statement is not yet available), from the beginning of gestation. The culture of those individuals who form his social environment is built into his body, into its tempo and its rhythms, into its sensitivities and its insensitivities. The reality of culture permeates the organism, and at no time can a human being, bred in a group of human beings, be said to stand outside culture, or to have personal experiences that are not culturally shaped. Such a statement as "hunger becomes appetite" is only relatively true, for even the hunger of the three-day old baby is already patterned by the feeding habits of its culture: whether a newly delivered mother gives her baby the breast; whether a child is fed intermittently but almost continuously; whether it is starved for three days, etc. Dr. Angyal uses the phrasing "cultural determinations are superimposed also on primary physical functions such as sex, food intake and excretory functions" (p. 187), but, as none of these physical functions are ever manifested by a human being who has not been at least prenatally moulded by contacts with other human beings, who themselves embody regularly patterned forms of behavior, we can not speak of culture being *superimposed* upon, but must speak rather of physical functions which develop in a culturally patterned way. A human being developing without contact with other human beings (if, for instance, it were possible for him to be reared by another mammal) would provide valuable data on original nature. But it is untenable to consider any human being, who have been reared among human beings, as having at any point in his life-history, or in any layer of his personality, or in any segment of his soma, some area somehow unpatterned and set vis-à-vis culture.[10]

The anthropological approach to the problem of personality means, in effect, that the anthropologist's task is to describe this over-all cultural setting, within which any given set of individuals, or a given individual, develops, and to identify and describe the process in which the individual expresses the regularities of the culture of which he is a member.[11] My original illustration of neck rests was at the very simplest level, a habit connected with an object of material culture. But, at each level of description, the anthropologist's task is to describe the way in which the culture must be taken into account. The term "personal experience" raises a problem of a more abstract level. Before the student of personality can study the effect of "personal experience," it is necessary for the anthropologist to say what order of personal experience individuals born within that culture can have. Travel, for instance, experience of another culture, may be something which members of one culture will be able to integrate and assimilate, deal with consciously, and thoroughly symbolize (in Angyal's sense), while, for members of another culture, it may merely involve a disorienting experience of completely unassimilable sensations, objects which can not be named or classified, or even subsequently remembered. For an American, who goes for a walk in the country and hears a stone dislodged from the bank along which he is walking, there is almost no chance that if the stone is too small to be a presage of accident of any sort, that it will become a "personal experience." For the Plains Indian, the same incident might become the basis of a "vision" which would provide a lifelong validation of his social position in his community. Attempts to collect life-histories from members of different cultures immediately reveal the most conspicuous discrepancies in the extent to which the idiosyncratic details of an individual's life have become personal experience, or have been, under a different cultural pattern, smoothed back into a stereotyped position. Iatmul natives will quite readily relate the story of their lives, fill them with incident and conversation which have all the ear marks of remembered events to which the individual personality might have been expected to have responded, but then continue the story, without a break, into some event where they were not present, or into a culturally stereotyped future. So an unmarried boy of eighteen may relate his marriage and the birth of his children. Balinese natives, on the other hand, born and patterned by a culture which treats time as circular, and lacking effective devices for handling temporal sequences of any length, will relate series of episodes, some highly stereotyped, some with an idiosyncratic flavor, but without time sequence. In other words, personal experience takes place with reference to an organism whose capacity for personal experience is already in that organism, both in terms of idiosyncratic endowments of sensitivity and "intelligence," and in terms of the whole modification which the organism has undergone as a member of a culture. The most personal, the most idiosyncratic experience that any individual can ever have, must be seen, for full understanding, as occurring within an acculturated person. It is not a question of thinking sometimes about the cultural aspect of a situation and sometimes of the personal, or sometimes of family culture and sometimes of wider culture, but of operating with the widest frame of reference always in mind. The anthropologist's task is, therefore, to provide this necessary widest frame of reference, to describe the relevant regularities which the student of personality will wish to take into account. This means that the anthropologist will collect "personal experience" as data upon the cultural regularities in terms of which events of an individual's life may be experienced, not as data about the personality of Ombomb of Aliton, or Nang Karma of Bajoeng Gedé, nor initially in order to make abstractions about the growth of personality which will transcend cultural lines. The anthropologist's first task is to organize material about individual versions of the culture so as to throw maximum light upon the culture, and the type of personality development possible, or probable, within that culture. But, with a series of cultures so studied and so analyzed, the anthropologist is then in a position to contribute materials and hypotheses upon forms of personality development, always, however, with his primary point of reference, the culture, shared by a number of members, rather than by the individual sharer of the culture.

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[10] See question raised by Dr. Hans Fried in discussion.

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[11] Bateson, Gregory. *Op. cit.*

There is, however, a still more abstract level at which the anthropologist can contribute to an attempt such as Dr. Angyal's, that is, to treat Dr. Angyal's whole statement as a cultural product and attempt to identify the way in which Dr. Angyal's hypotheses are themselves the product of his culture, and to what extent they are limited by the culture. When this is done, Dr. Angyal's basic antithesis between the trend toward autonomy and the trend toward homonomy will be seen to be characteristic formulations within Western European culture, formulations which are not inherent in the nature of life, or in the nature of man-in-culture, but simply part of our cultural approach to the world. Not only is this antithesis, which is a rewording of older forms of antithesis between man and society, one which individuals reared in our own culture tend to create, but the particular form in which he expresses it, in which the social tendencies are subsumed under a tendency to submit, to passively become part of a "super-individual unit," is relevant to Euro-American culture at the present time,—to totalitarian theories of the state, and to the whole phenomena of fascism in its position counterposed to individualism, itself conceived as an extension of, or expension of, individual power over the environment. Such a formulation grows naturally out of a culture where goal-oriented activity and emphasis upon power over persons and things, including one's own life, is vigorously symbolized. However, in cultures like the Balinese, such a formulation would be irrelevant and non-descriptive of Balinese behavior.[12] Another of Dr. Angyal's statements, that "The organization of the attitudes of a person is essentially a hierarchical one" (p. 143), reflects our own cultural pattern, but would not be relevant to Iatmul culture, in which the concept of hierarchy, either in overt social forms or the corresponding character organization, is lacking.[13]

This question of the importance of understanding the extent to which hypotheses are culturally limited or restricted, and the related problem of how much they owe, for their richness and insight, to any particular culture, is one of very special importance for all of those sciences which deal with human beings, in which the observer *must* be included inside the experiment. While in the physical sciences, it is possible to work directly to hypotheses which, when verified, may be used, at either the theoretical or applied level, by members of other cultures than that in which the original work was done, the question of borrowing scientific hypotheses in the human sciences becomes much more complicated. It is probable that only after we have developed a series of culturally limited and culturally rich hypotheses in a number of cultural settings will we have enough material actually to build a system which is genuinely cross-cultural and includes, or constructively excludes, the many different cultural frameworks now present in the world. In the meantime, it is a function of the anthropologist to submit such frameworks as are developed to cross-cultural analysis and to point out the extent to which such cultural limitations and enrichments do occur.

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[12] Bateson, Gregory. The frustration-aggression hypothesis and culture. Psychological Review 48 (4): 350–355. 1941.

Bateson, Gregory. "Balinese Character: a photographic analysis." Bateson and Mead. Special publication of The New York Academy of Sciences. 2: 277 pp. December, 1942.

[13] Mead, Margaret. Administrative contributions to democratic character formation at the adolescent level. Jour. Nat. Asso. of Deans of Women. 4(2): 51–57. 1941.

DISCUSSION

Dr. Ernst Kris emphasized the contribution which anthropological studies should be able to make to the problem of the limits of human variability. Other points discussed included a problem, raised by Dr. Hans Fried, that if we rejected Dr. Angyal's category of "adjustment to culture," we rejected an important point of leverage for social change, as, for instance, in German assumptions about military training which postulated a human unwillingness to be cruel which had to be overcome by training. Dr. Mead replied that, where a culture included in its phrasing of human nature the assumption that part or all of the cultural behavior was antithetical to human beings' natural tendencies, then such a phrasing did give us a valuable point of leverage for cultural change. Such an idea of "adjustment," however, was not a cross-cultural abstraction. Another questioner asked how it was possible to use the concept of culture, as based on the study of homogeneous, relatively static cultures, under modern world conditions, where husband and wife might come from radically different cultures and rear their children in a third. Dr. Mead replied that the behavior of such people would be no less systematically related to their experience, but that a higher level of abstraction, one defining the degree of heterogeneity, would be necessary. She cited the lack of expressive movement among Americans as a possible manifestation of heterogeneity, growing out of the handling which children receive when reared by representatives of several different cultures. A misunderstanding was cleared up about the need for developing anthropologists from each culture to study their own culture. Dr. Mead explained that she meant that we needed the culturally enriched contributions to scientific theory, not the specific observations of anthropologists on their own cultures. A question was raised from neurology, as to whether the science of culture and personality had not now reached a period analogous to the period in medicine when symptoms, on the one hand, and the nature of the bacteriological environment, on the other, were both known, but a knowledge of the dynamic mechanisms was still lacking. Dr. Mead replied that anthropologists approached the problem of dynamics through a study of the growth of the child within a culture, which was her usual method of approach, but that Dr. Angyal's presentation had not evoked discussion of this method. Mr. L. K. Frank, as chairman, brought out two points: (1) A culture may be viewed somewhat like the more recent conception of a gas, as composed of a large number of molecules which only approximate to the pattern of behavior appropriate to their chemical composition, but, nevertheless, collectively give rise to the orderly, regular behavior we call chlorine or hydrogen. In a culture, we have the most frequent modal patterns of conduct, with deviations toward the criminal extreme and toward the psychotic extreme. Individuals vary in their conformity to the presented or modal patterns of a culture, according to their constitutional-temperamental readiness for such patterns, and, also, according to the degree and kind of culturizing tuition to which each has been subject. (2) In an intact, homogeneous culture, we have a unimodal distribution of conduct. In a culture that is becoming diversified and heterogeneous, such as ours today, we have a multi-modal culture. The question, then, is: What does conformity mean in such a multi-modal culture and what criteria of "adjustment" should be used?

Dr. Angyal concluded the discussion by saying that this preliminary exploration of the literature on primitive cultures had not led him to believe that his basic hypotheses would have to be modified.

REPORT OF THE ANNUAL MEETING

DECEMBER 15, 1943

The 126th Annual Meeting of the Academy for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at The American Museum of Natural History on the evening of Wednesday, December 15.

The Corresponding Secretary reported that there are now 44 Honorary Members and 1 Corresponding Member upon the rolls of the Academy. Four deaths were reported during the past year.

The Recording Secretary's report stated that, during this second year of war, the Academy has proved its fundamental stability and usefulness by the complete continuance of its progressive program with especial emphasis upon its conferences and other scientific meetings and through the active publication of their results in a series of notable documents.

That these results are appreciated by the members of the Academy is attested by their active support and even by an actual increase in the membership total during 1943. A noteworthy evidence of the interest of the members in the Academy activities has been the cordiality with which they now assume the responsibility for expansion of Academy membership.

The Academy has held 28 Regular Sectional Meetings, at which 28 stated important scientific papers were presented. This is in addition to two excellent addresses delivered on the occasion of the Annual Business Meeting.

Ten informal receptions were held under the auspices of the various Sections of the Academy.

Five two-day conferences on special subjects of research were held during the year. These meetings were attended by outstanding investigators in these fields. The titles of the conferences are as follows: Section of Biology, "Parasitic Diseases and American Participation in the War"; Section of Physics and Chemistry, "High Polymers," "Sulfonamides," "Magneto Chemistry"; Section of Psychology, "Psychosomatic Distrubances in Relation to Personnel Selection."

Four Honorary Members, 4 Life Members, 124 Active Members, 35 Associate Members and 10 Student Members were added to the rolls, of which 163 are now in good standing and 14 await qualification through payment of dues. Thus, a total of 177 new Members were added during the year.

There are at present upon the rolls of the Academy 1,985 members, of whom 260 are Fellows. These include 1 Patron, 95 Life Members, 123 Sustaining Members, 1,089 Annual Active Members, 603 Associate Members, 29 Student Members, 44 Honorary Life Members and 1 Corresponding Member.

The Editor reported that, during the fiscal year of 1943, The Academy published 872 pages. Of this amount, 670 pages were published in the Annals and 202 pages in the Transactions. In addition to the articles already published, Articles 2, 3 and 4 of Volume 45 are being paged and Article 5 is in galley proof. These are all scheduled for publication before the end of this calendar year. The illustrations for Volume 45, Article 6, have been engraved, the manuscript of the text is now in the Editor's hands, and the completed article will appear early in 1944. The manuscript of Volume 12, Part 4, of the Porto Rico Survey, is also being revised by the Editor and will be printed early in 1944. This will complete Volume 12 of the Survey.

The detailed list of the publications is as follows:

ANNALS

Volume 43, Article 6—"Protein Hormones of the Pituitary Body," by H. B. van Dyke, Bacon F. Chow, Vincent du Vigneaud, H. L. Fevold, George W. Irving, Jr., C. N. H. Long, Theodore Shedlovsky, and Abraham White. Pages 253–426. Published February 26, 1943.
This completes Volume 43.

Volume 44, Article 1—"Boundary-Layer Problems in the Atmosphere and Ocean," by C. G. Rossby, B. Haurwitz, Benjamin Holzman, Woodrow C. Jacobs, A. A. Kalinske, Phillip Light, R. B. Montgomery, and H. U. Sverdrup. Pages 1–104. Published May 29, 1943.

Volume 44, Article 2—"Criteria for Vertebrate Subspecies, Species and Genera," by Charles M. Bogert, W. Frank Blair, Emmett Reid Dunn, E. Raymond Hall, Carl L. Hubbs, Ernst Mayr, and George Gaylord Simpson. Pages 105–188. Published June 8, 1943.

Volume 44, Article 3—"Parasitic Diseases and American Participation in the War," by Horace W. Stunkard, Lowell T. Coggeshall, Thomas T. Mackie, Robert Matheson, and Norman R. Stoll. Pages 189–262. Published September 30, 1943.

Volume 44, Article 4—"High Polymers," by Raymond M. Fuoss, J. Abere, W. O. Baker, Henry Eyring, John D. Ferry, Paul J. Flory, C. S. Fuller, G. Goldfinger, R. A. Harman, Maurice L. Huggins, H. M. Hulburt, H. Mark, H. Naidus, Charles C. Price, John Rehner, Jr., Robert Simha, and A. V. Tobolsky. Pages 263–444. Published November 12, 1943.

Volume 44, Article 5—"Sulfonamides," by Colin M. McLeod, Paul H. Bell, Henry Irving Kohn, J. S. Lockwood, Richard O. Roblin, Jr., James A. Shannon, and H. B. van Dyke. Pages 445–538. Published December 14, 1943.

Volume 44, Article 6—"Psychosomatic Disturbances in Relation to Personnel Selection," by Lawrence K. Frank, M. R. Harrower-Erickson, Lawrence S. Kubie, Gardner Murphy, Donal Sheehan, and Harold G. Wolff. Pages 539–624. To be published December 22, 1943.
This will complete Volume 44.

Volume 45, Article 1—"The Relation Between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails," by Arthur W. Pollister and Priscilla F. Pollister. Pages 1–48. Published November 30, 1943.

Volume 45, Article 2—"Lycaenidae of the Antilles," by William P. Comstock and E. Irving Huntington. Pages 49–130. To be published December 29, 1943.

Volume 45, Article 3—"Stellar Dynamics," by S. Chandrasekhar. Pages 131–162. To be published December 31, 1943.

Volume 45, Article 4—"Studies on Fresh Water Bryozoa," by Mary D. Rogick. To be published December 31, 1943.

Volume 45, Article 5—"The Social Behavior of the Laughing Gull," by G. K. Noble and M. Wurm. To be published December 31, 1943.

Volume 45, Article 6—"Experimental Modification and Control of Molts and Changes of Coat-Color in Weasels by Controlled Lighting," by Thomas Hume Bissonnette and Earl Elmore Bailey. To be published early in 1944.

TRANSACTIONS

Series II, Volume 5, Nos. 1–8, consisting of 202 pages, was completed, printed and distributed each month from November, 1942 to June, 1943, inclusive.

In September, 1943, Doctor Wilbur G. Valentine, the able Editor of the Academy publications, was called into Government service, much to the regret of the Council, and Doctor Roy Waldo Miner was appointed Acting Editor by that body to carry on the editorial work from October 12th to the end of the year. Mrs. Ethel J. Timonier has acted as Assistant Editor.

The Librarian reported that, in response to requests and orders, 19,416 separate publications, and 1,909 complete volumes of Transactions, Series II, were distributed as follows: *Annals,* 13,068; *Scientific Survey of Porto Rico and the Virgin Islands,* 888; *Special Publications,* Volume I, "Climate and Evolution," 73; Volume II, "Balinese Character," 525; and *Transactions,* Series II, Volume 5, complete volumes, 1,909, and 4,862 single copies.

The Library of the Academy has received, from exchange institutions, 977 separate publications.

The Treasurer reported that The Academy has continued to maintain a sound financial status in spite of the conditions affecting similar institutions during the war period.

The total income received from all sources amounted to $25,429.16. This is an increase of $2,678.85 above that of 1942.

Through the hearty cooperation of its membership, the Academy added 177 new names to its rolls, thus adding a slight gain of $88.55 to our revenue from membership dues, making a total of $8,345.00 from this source.

Receipts from sales of publications amounted to $5,423.16, an increase of $2,317.50 over that of last year.

The Council of the Academy took advantage of an opportunity to sell a real estate property which had been held for many years and which had been carried at a considerable loss. This resulted in a reduction of the book value of the Academy's holdings, but relieved the income-producing investments to such a degree that instead of yielding 3.5% as in 1942, during 1943 they produced 5.5%, giving a substantial net increase from this source.

The book value of the Academy's property, as of November 30, 1943, now amounts to $84,878.61; of which $68,254.10 is in stocks and bonds; $895.04 is a mortgage bond; and, the remainder, $15,729.47, is represented by cash in bank, a portion of which results from the redemption of bonds and is awaiting reinvestment.

Following the usual custom, the books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1943, and have been audited by the Finance Committee, as provided by the Constitution and By-Laws.

The property of the Academy was verified and the Treasurer's report examined and attested by the Finance Committee as of December 14, 1943.

The A. Cressy Morrison Prizes of two hundred dollars each, for the two most acceptable papers in any field of natural science, within the scope of the Academy and its Affiliated Societies, were awarded to the following papers,[1] entitled:

"The Pre-Iroquoian Occupations of New York State," by William Augustus Ritchie, Rochester Museum of Arts and Sciences, and "The

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[1] Abstracts of these papers are included in this issue of the Transactions.

Distribution of the Salamanders of the Genus *Plethodon* in Eastern United States and Canada," by Arnold Grobman, University of Rochester.

The committee desires to congratulate the authors of these papers and to express its appreciation of this method of furthering the advancement of science.

The following members were elected to Fellowship:

John M. Arthur, Ph.D.
Hugh D. Auchincloss, M.D.
Charles O. Beckman, Ph.D.
Alexander W. Blain, M.D., M.S.
Aaron Bodansky, Ph.D.
Oscar Bodansky, Ph.D.
Richard M. Brickner, M.D.
Mary L. Caldwell, Ph.D.
Leslie A. Chambers, Ph.D.
Herbert C. Clark, M.D.
Kenneth S. Cole, Ph.D.
Cornelius J. Connolly, Ph.D.
William H. Cowley, Ph.D., LL.D.
Lyman C. Craig, Ph.D.
M. L. Crossley, Ph.D.
David Davidson, Ph.D.
E. F. Degering, Ph.D.
Rene Jules Dubos, Ph.D.
Gioacchino Failla, D. Sc.
Margaret C. Ferguson, Ph.D., Sc.D.
Erich Fromm, Ph.D.
Joseph T. Gregory, Ph.D.
Ernst A. Hauser, Ph.D.
Philip Bovier Hawk, Ph.D.
Edna Heidbreder, Ph.D.
Kenneth C. D. Hickman, Ph.D.
Charles L. Hoagland, M.D.
Edward J. Kampf, M.D.
Bostwick H. Ketchum, Ph.D.
James H. Kimball, Ph.D., D.Sc.
Herbert S. Langfeld, Ph.D.
Eugene J. Lorand, D.E. (Budapest)
Max Bernard Lurie, M.D.
Leland S. McClung, Ph.D.
Warren Sturgis McCulloch, M.D.
Walter Christian Michels, Ph.D.
Ralph S. Minor, Ph.D.
Stanley O. Morgan, Ph.D.
Walter M. Nielson, Ph.D.
Peter K. Olitsky, M.D.
Alwin M. Pappenheimer, Jr., Ph.D.
Edward G. Pickels, Ph.D.
Robert N. Pease, Ph.D.
Hortense Powdermaker, Ph.D.
Anne Roe, Ph.D.
Mary D. Rogick, Ph.D.
William T. Salter, M.D.
Julius M. Rogoff, Ph.G., Sc.D., M.D.
James Plummer Schooley, Ph.D.
Gotthold Steiner, Ph.D
J. G. Phelps Stokes, M.D.
W. Duncan Strong, Ph.D.
Ernst Weber, Sc.D., Ph.D.
John Warren Williams, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists[1]:

Oswald Theodore Avery, Sc.D., M.D., LL.D., Bacteriology, Member, Rockefeller Institute for Medical Research, New York, N. Y.

Alexander Fleming, M.B., B.S. (London), F.R.C.S. (England), Bacteriology, discoverer of penicillin; Professor of Bacteriology, University of London, St. Mary's Hospital Medical School; Hunterian Professor and Arris and Gale Lecturer, Royal College of Surgeons.

Sir Frederick Gowland Hopkins, M.A. Cantab., M.B., D.Sc. (London), Hon. Sc.D., London, Oxford, Manchester, Dublin, Sheffield, Glasgow, Leeds, Reading and Harvard; LL.D., Birmingham, Aberdeen and St. Andrews; D.C.L., Durham; Hon. M.D., Louvain, Budapest, Lausanne; Biochemistry, Professor, University of Cambridge, England; Sir Williams Dunn Professor since 1921.

Alfred L. Kroeber, Ph.D., Anthropology, Director, Museum of Anthropology, University of California, Berkeley, California.

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[1] The class of Honorary Life Membership of The New York Academy of Sciences is limited to fifty..

Th. Svedberg, Fil. Kand., Fil. Lit., Fil. Dr. (Uppsala); Dr. Chem. (Uppsala); Hon. Dr., Wisconsin, Uppsala, Harvard, Delaware, Oxford; Chemistry, Developer of the Svedberg Ultracentrifuge Apparatus, Professor and Head of Physical Chemistry Institute, University of Uppsala, Sweden.

Arne Tiselius, W.K., Fil. Mag., Fil. Lit., Fil. Dr., Dr. Chem. (Uppsala), Biochemistry, Application of electrophoresis to proteins; Professor of Biochemistry, University of Uppsala, Sweden.

The following officers were elected:

For President
DUNCAN A. MACINNES

For Vice-Presidents

G. MARSHALL KAY
ANNE ROE
HORTENSE POWDERMAKER
RALPH H. CHENEY
LEWIS G. LONGSWORTH
RAYMOND B. MONTGOMERY

For Recording Secretary
MICHAEL HEIDELBERGER

For Corresponding Secretary
H. HERBERT JOHNSON

For Treasurer
DONALD BELCHER

For Librarian
BARNUM BROWN

For Editor
ROY WALDO MINER

For Councilors (1944–1946)
VICTOR K. LAMER
WALTER H. BUCHER
THEODORE SHEDLOVSKY

For Finance Committee
HARDEN F. TAYLOR, *Chairman*
HARRY B. VAN DYKE
ADDISON WEBB

After the Business Meeting, the following program was presented:

Address by the Retiring President

"How Do Tapeworms of Herbivorous Animals Complete Their Life Cycles?"
by
HORACE W. STUNKARD

"The Arctic Mediterranean"[1]
by
VILHJALMUR STEFANSSON

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[1] No abstract of this address was received.

THE NEW YORK ACADEMY OF SCIENCES

Address of the Retiring President[1]
given at Annual Dinner and Meeting
December 15, 1943

Professor Horace W. Stunkard, Department of Biology, New York University, New York, N. Y.: *How Do Tapeworms of Herbivorous Animals Complete Their Life Cycles?* (This address was illustrated by lantern slides.)

It has been said that Nature follows dark and devious paths her wonders to perform. The truth of that statement seems more and more apparent, the more we know of the intricacies of biological phenomena. Certain observers discover the wonders of Nature only in far-distant climes, but to the illumined mind and observant eye—they are all about us—right in our own gardens. At the annual meeting of the Academy six years ago, I reported on the life histories of certain digenetic trematodes, tracing the alternation of generations and the transfers of hosts that take place in these involved and complicated life cycles. The account had all the aspects of a series of fairy tales, but the stories were true in general outlines and in the details.

I wish tonight to explore with you other dark and devious paths followed by other parasitic worms in the completion of their life histories. The cestodes or tapeworms are common intestinal parasites of all classes of vertebrates. In their several hosts, from the most primitive fishes to human beings, they manifest much variation in form. Typically, the cestode in the intestine consists of a scolex or head with an attached string of proglottids or segments which constitutes the strobila. The scolex bears certain organs, suckers or hooks, or both, by which it attaches to the wall of the intestine. The strobila may be short and consist of only a few segments, but usually it attains considerable length, often ten feet or more, and is composed of hundreds or thousands of proglottids. The region near the head is a zone of proliferation and growth, where new segments are formed. As these segments become progressively older, they proceed backward in the chain and the oldest ones are at the terminal end where, in taenioid species, they become detached and pass out of the intestine. Somewhere near the middle of the strobila, each proglottid develops reproductive organs, testes and ovaries; eggs are produced and fertilized and then pass into a uterine sac. The reproductive organs then degenerate and disappear; the eggs begin to develop and the terminal proglottids are filled with mature eggs. The eggs are microscopic. In different species, they vary in size from 30 to 80 microns, and each one contains a motile, six-hooked larva, known as an onchosphere. These larvae are exceedingly small, averaging about 20 microns in diameter. Thus, in the intestine, the tapeworm has two forms of multiplication: one, asexual, by the production of new proglottids; and the other, sexual, with the production of eggs and their development to the first larval stage.

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[1] Dr. Stunkard served as President of the Academy during the years 1942 and 1943.

But this is only half of the story. The tapeworm leads a double life. The larvae, encased in the eggs, pass out from the intestine of the host, but, with the rarest of exceptions, they are unable to infect the same host-species again. They can live for long periods within the protective covering of the egg shells, and never leave the shells unless they are ingested by some other animal. When they are eaten by an animal which can serve as an intermediate host, they emerge from their shells, leave the intestine, undergo metamorphosis, and grow into second stage larvae, which are then infective for the original host. These larval stages occur either in the body cavities of invertebrate animals or in cystic vesicles attached to or embedded in the tissues of vertebrates. With rare exceptions (the hydatid cysts), multiplication does not take place in the intermediate host. The tapeworm simply transforms from a first to a second larval stage and an intermediate host serves primarily for the metamorphosis of the parasite to a stage where it is able to continue its development when introduced into the intestine of the original host. In the more primitive cestodes, those of lower vertebrates, there are usually three hosts in the life cycle, that is, first and second intermediate hosts which harbor successive larval stages, and the second intermediate host is eaten by the final host where the worms are liberated, attach themselves to the intestinal wall and become sexually mature.

Most of the species are large enough to catch the eye of any alert observer. They have been known since ancient times and Aristotle recorded both the sexually mature strobilate stages and the cystic bladder-worm stages; although at that time there was apparently no suspicion that these forms constitute successive stages which alternate with each other in the life cycle single species. The description of a large number of tapeworms led to the formulation of scheme of classification by Zeder (1800). This author called the vesicles "Blasenwürmer," and proposed the name *Cysticercus* for what we now know to be the larval stage of *Taenia hydatigena*, a tapeworm of the dog.

Gradually, it became apparent that there was some connection between these bladder-worms and tapeworms. Malpighi (1698) had discovered the scolex in the cysticercus of the pork tapeworm. Pallas (1760) and Goeze (1782) described the scolices in the cysts of other species. The latter author noted the agreement between the scolex of the cat tapeworm and the scolex of the bladder-worm found in the liver of the rat and mouse. In this species, the scolex begins to proliferate and produce proglottids while still in the cyst. Goeze (1782) had observed embryos in the eggs of the dog tapeworm, *Dipylidium caninum.* Von Siebold (1835) and Dujardin (1837) described the six-hooked onchospheres and their movements, but the relation between these larvae and the bladder-worms was still obscure. Von Siebold (1844) predicated that bladder-worms represent hydropsical, degenerate worms that had been introduced accidentally into abnormal hosts. Although he still held that the cysticercus was an abnormal or teratoid form, von Siebold (1850) recognized that cysticerci were derived from onchospheres by metamorphosis. He stated, p. 209, "Diese Embryone (onchospheres) müssen jedenfalls noch eine Metamorphose erleiden, durch welche sie erst eine dem Kopfende der Taenien oder Bothriocephalen entsprechende Gestalt bekommen." Steenstrup's (1842) studies had called attention to the alternation of generations in lower invertebrates; whereupon Blanchard (1847) and van Beneden (1849) suggested that the bladder-worms are normal stages in the life cycle of tapeworms. The first step in the final solution of the problem was made by Küchenmeister (1851, 1852) who fed bladder-worms (*Cysticercus pisiformis*) from the mesenteries of rabbits to dogs and obtained adult tapeworms, *Taenia serrata.* This experiment demonstrated that these two forms, previously regarded as distinct species, were really only different and successive stages in the life history of a single one. Shortly thereafter, van Beneden (1854) discovered the life cycle of *Taenia solium* of man. He fed eggs of the tapeworm to pigs and obtained the larvae, *Cysticercus cellulosae,* long known as the cause of "measly" pork. Following other successful experiments on the life cycles of cestodes of dogs and cats, Leuckart (1862) discovered the life history of *Taenia saginata,* the human beef tapeworm. He fed eggs to calves and recovered the larvae, *Cysticercus bovis.* Other similar life cycles were experimentally demonstrated, but the next significant advance was made when Grassi and Rovelli (1889, 1892) showed that the larvae of the dog tapeworm, *Dipylidium caninum*, develop in the dog flea, and that they are not bladder-worms, but minute, solid-bodied forms with a small and transitory vesicle for which the term "cysticercoid" was used by Leuckart. In this case, the tapeworm eggs are eaten by the larvae of the flea. Grassi and Rovelli also showed that in *Hymenolepis nana* var. *fraterna* of rats, when eggs are ingested by rats and mice, the onchosphere larvae emerge, bore into the wall of the intestine, and complete their larval development there. When they have attained the cysticercoid stage, which is infective for the final host, they return to the lumen of the intestine, attach themselves, and grow into the sexually mature cestodes. Here, no intermediate host is necessary and the cestode can complete its life cycle in a single host species. More recently, Bacigalupo (1928, 1931) has shown that larvae of this rat tapeworm may develop in the body of the meal worm, *Tenebrio molitor*, and other insects, so the direct development in the intestinal wall of the rat or mouse probably represents an abbreviated type of life history in which the intermediate host has been dropped out of the cycle. This brief survey will serve as a background for the investigations which I wish to discuss tonight.

The strobilate stages of cestodes occur chiefly in carnivorous hosts, selachian and teleost fishes, amphibians, reptiles, birds and mammals, and these animals acquire the infection by eating other animals in which the larval stages are present. All of these vertebrates feed, either regularly or occasionally, on other animals, and even seed-eating birds supplement their diet with worms and insects.

It has long been known, however, that herbivorous animals, cattle, sheep, deer, horses, rhinoceroses, rabbits, monkeys, etc., are commonly and often heavily infected with adult cestodes. The worms are most abundant in the ungulates (hoofed animals), but are common also in rodents and marsupials. The tapeworms characteristic of herbivorous animals belong to a single zoological family, the Anoplocephalidae. Thé name is derived from *Anoplocephala perfoliata*, the giant tapeworm of the horse. A monograph of this family by Baer (1927) showed that at that time it contained 114 species, arranged in 28 genera and distributed as follows: 84 species in mammals, 17 species in birds, and 13 species in reptiles. They are common in domestic animals and, since sheep and cattle have been taken to all parts of the world by human migrations, their parasites have been taken with them and are cosmopolitan in distribution. These domestic animals, cattle, sheep and horses, especially the younger individuals, may be seriously affected by these parasites. Several hundred specimens of *Moniezia expansa*, the common tapeworm of sheep, have been taken from a lamb 4–5 weeks old and, since the worms measure from 1 to 5 meters in length and up to 1.5 cm. in width, the intestine may be practically occluded by them. Another species parasitic in sheep, *Thysanosoma actinioides*, invades the liver and blocks the biliary passages. The pathology and symptomology of infection is recorded in standard textbooks of veterinary medicine. In view of the economic importance of these parasites, diligent search has been made for more than 75 years to determine the method by which they are acquired. The Bureau of Animal Industry in the United States Department of Agriculture has conducted experiments during practically all of this time. More than 50 years ago Curtice (1889, 1890), Stiles and Hassall (1893), and Stiles (1901) reported studies undertaken to discover the life history of the tapeworms of sheep and cattle. Similar studies were carried on in England, France, Germany, Russia, Italy, South Africa and Australia. Professor Leuckart, of Leipzig, regarded as *der Altmeister der Helminthologie*, and several of his students, devoted many years to the problem. One of these students, Riehm (1881), reported that the hares and rabbits of North Germany harbor five different species of anoplocephaline cestodes and, although extended observations and experiments were made, they failed to disclose the method of transmission. In the ensuing 60 years, a host of investigators has proposed all kinds of hypotheses, some fantastic, to explain the life cycle and method of transmission of these parasites. An examination of the literature covering this period discloses indefinite, contradictory and negative results. Opinion shifted from one point of view to another as repeated efforts failed to discover the life history. Many important observations were recorded, but few of the experiments were sufficiently well controlled to yield trustworthy results.

The repeated failures served to stimulate increasing interest in this baffling problem which came to be regarded as the great enigma of helminthology. It seemed preposterous and incomprehensible that every rabbit and sheep in a pasture could harbor tapeworms and that it was impossible to find out how they became infected. My work on this subject was begun in 1929. Since larger animals could not be used for laboratory studies, and since it would be difficult to control experiments on infection of such animals, I began with tapeworms of rabbits. Cestodes and eggs with their contained onchosphere larvae were obtained from wild rabbits and the experiments conducted with domestic laboratory-raised specimens. Since there were a few reports of cestodes in domestic rabbits that had been allowed to range in the fields, it seemed probable that they could be infected in the laboratory. The food of rabbits can be carefully examined and such experiments conducted under conditions of rigid control.

Fortunately, sabbatical leave from New York University and a fellowship from the Guggenheim Foundation permitted a year (1931–1932) of uninterrupted study. The domestic rabbit was derived from the wild European species, *Oryctolagus cuniculus,* and is distinct from the wild American rabbit which belongs to the genus *Sylvilagus.* Furthermore, the tapeworms of the European wild rabbit are specifically distinct from those of the American wild rabbit. It seemed probable, therefore, that the domestic rabbit would be more susceptible to infection by tapeworms of the European rabbit than to those of the American wild rabbit. Riehm (1881), in the monograph cited earlier, had reported that the rabbits and hares of North Germany harbor five different species of tapeworms and so that region seemed the ideal one in which to conduct the experiments. The facilities of the Institut für Schiffs- und Tropenkrankheiten at Hamburg were generously placed at my disposal by the Director, the late Professor Doctor Fülleborn. Work was carried on there for the year. It was proved definitely that direct infection from one rabbit to another is impossible. Eggs containing living, moving larvae were fed to rabbits and were always recovered in the feces. They did not hatch and the larvae in them were alive and motile after passing through the digestive tract of a rabbit three or four times. Aging of the eggs and exposure to all kinds of environmental conditions did not affect the result. It was noted, however, that, if the shell of the egg was mechanically injured, the larva would emerge These larvae measure approximately 0.02 mm. in diameter. Young tapeworms as small as 0.3 mm. in length were recovered from the intestine of naturally infected wild rabbits. The smallest ones consisted essentially of a small but perfectly formed scolex and it was clear that when first recognizable in the rabbit, the parasite had changed entirely from the onchosphere larva and had increased some 15 to 20 times in diameter. This growth and change of form must have occurred in an environment where protection and nourishment were available, that is, in the body of some intermediate host. Epidemiological considerations made it possible to delineate that host with much clearness.

In the report of the work at Hamburg, I (Stunkard, 1934) made the following statement: "The intermediate hosts of anoplocephaline cestodes must be terrestrial invertebrates which in either larval or adult stages frequent the fecal material of the animals which harbor the sexually mature cestodes. Ordinarily, the eggs are very abundant in the feces and, if the intermediate host were to pick them up and feed on them, it would probably be unable to survive the severity of the infection. The ingestion of the onchospheres therefore is almost certainly accidental or incidental in the feeding habits of the intermediate host. It is possible that the intermediate host may acquire the parasites during only a portion of the year or during a certain period of its life cycle, but, once acquired, there is much evidence to indicate that this host will carry the infection for at least a year. The host is present and infective throughout the year, although much more abundant in summer than in winter. In view of the high incidence of infection and the large number of specimens that frequently occur in the intestine of a host (several hundred worms in the intestine of a lamb four weeks old), the intermediate host must be extremely abundant. It must be inconspicuous and very small (the intermediate host of *Andrya sp.*, e.g., may be accidentally eaten by a rabbit 2–3 weeks old). If, as believed, lambs become infected with *Moniezia* before they feed on vegetation, the intermediate host must not only be present on vegetation but actually enter the mouth of the animal. A large proportion of the definitive hosts feeds at night and the intermediate host may be a nocturnal species. Since the family Anoplocephalidae contains over one hundred known species, it may be expected that the intermediate hosts belong to a large family. Furthermore, this family must be world-wide in distribution and its members be more or less regularly eaten by herbivorous mammals, man, monkeys, cattle, horses, rhinoceroses, rodents, etc. These intermediate hosts are eaten also by birds and reptiles. The number of invertebrate animals which satisfy all of these requirements is small enough that the discovery of the life cycle may be confidently expected."

Since the onchospheres emerge from the eggs when the shells are mechanically injured, it seemed probable that the intermediate host was mandibulate or provided with a grinding stomach. Ants appeared to fulfill all the requirements and thousands of them were taken from infected areas and dissected in the hope of finding the cysticercoid stage of the parasite. In addition, collections were made with a fine net, sweeping the vegetation in these infected areas at every hour of the day and night, and all of the animals thus taken were examined,—with only negative results. The year in Germany, although it provided much information, had not solved the problem.

The work was continued in New York and concentrated on the study of ants as possible intermediate hosts. I am indebted to Dr. W. S. Creighton of the City College for assistance in the collection, identification and housing of ants. The discovery by Jones and Horsfall (1935) that ants serve as intermediate hosts of chicken cestodes increased the suspicion that some ant transmitted the rabbit tapeworms. When dissection of ants collected in the field was fruitless, colonies of the common, smaller species were brought into the laboratory and fed the eggs of rabbit and sheep cestodes. But no infection resulted and it was apparent that the intermediate hosts were to be sought elsewhere. After six years of intensive effort, I was still groping in the darkness.

A fortunate combination of circumstances led to the discovery of the life cycle, and it was not the rabbit tapeworm, but the one from sheep, which yielded the first successes. When the experiments with ants were abandoned, the situation was desperate. It happened at that time that Doctors Ludwig and Fox of our laboratory, who had been studying Japanese beetle larvae, were annoyed by tyroglyphid mites that were overrunning their cultures. These mites had biting mouthparts and so eggs of both rabbit and sheep cestodes were fed to them. Dissection of the mites was begun several days later and, to my surprise and delight, larvae of both species of tapeworms were present in the body cavities of the mites. The eggs had hatched, the larvae had migrated to the haemocoele, but no development had occurred. The structure and bionomics of these mites precluded any hope that they were the long-sought intermediate hosts, but the preliminary success indicated that at last we were on the right trail. Many other kinds of free-living mites were collected from areas where sheep had not been allowed to graze and exposed to infection. The oribatid or beetle mites seemed to satisfy all the requirements postulated for the intermediate hosts and they proved susceptible. Eggs of both species of cestodes were fed to them, larvae were recovered from the body cavities, and, in members of the genus *Galumna,* the larvae of the sheep tapeworms began to develop. Dissection of mites, at varying intervals after exposure, showed much development of the larvae, but, as a result of high mortality among the mites and the use of immature cestode eggs in some of the experiments, no mature cysticercoid larvae were obtained in the dissections of the first year. A group of 200 galumnid mites was fed eggs of *Moniezia expansa* and, after four months, they were fed to a lamb which had been kept for several months in our animal house. The lamb later passed cestode eggs in the feces, which showed that the mites had transmitted the infection, but the infective stage of the parasite had not yet been observed. In later experiments, the use of specially designed nests reduced mortality of the mites and care was taken to use only mature eggs for infection. During the summer of 1937, a series of developmental stages from the onchosphere to the fully formed cysticercoid larva was recovered from experimentally infected mites and the life cycle of this species was announced (Stunkard, 1937). A more complete account with photographs of selected stages of living larvae was published the following year (Stunkard, 1938). The results have been confirmed subsequently by the studies of Stoll (1938), Krull (1939) and Shorb (1939). Krull reported that the mites do not actually eat the eggs. Instead, they cut a hole in the shell and ingest the contents.

Meanwhile, six years had elapsed since the work in Germany, another sabbatical leave was at hand, and the support of the Oberlaender Trust made it possible to return to Hamburg and renew the study of the tapeworms of rabbits. The discovery of the life cycle of the cestode of sheep and the experience of the previous year in the "Tropeninstitut" served to orient the investigation. Wild rabbits are not readily obtained during the summer and early autumn but, when the hunting season opens, they are abundant in the markets. Accordingly, the early weeks were devoted to the collection and dissection of mites taken from areas where the infection was heavy. If cysticercoid larvae could be found and fed to young domestic rabbits, and if infection resulted, the cestode could be identified and its natural host would be known. Such information would be particularly valuable later when eggs of the cestodes were to be fed to possible intermediate hosts. Representatives of mites common in the Hamburg area were identified by Doctor Max Sellnick of Königsberg.

Over 6000 mites, representing 38 different species, were dissected between July 27 and November 1, 1938. On September 8th, dissection of a specimen of *Scutovertex minutus,* an oribatid mite, yielded a fully formed cysticercoid which closely resembled those of the sheep tapeworms. It was 0.2 mm. in diameter and the scolex in it was alive and active. A small hole was bored with a needle in a grain of wheat, the larva was placed in it and the wheat was pushed down the throat of a half-grown, laboratory-raised, domestic rabbit. Examination of feces from the rabbit first disclosed cestode eggs on November 24th and they were abundant two days later. The animal was then killed and a single, mature, specimen of the rabbit cestode *Cittotaenia ctenoides* was found in the intestine (Stunkard, 1939). Eleven weeks had elapsed since the larva was fed to the rabbit. Other cysticercoids were found in the same and other species of oribatid mites and fed to laboratory rabbits, but no more sexually mature tapeworms were recovered. The other cysticercoids may have been immature or they may have belonged to species which do not develop in rabbits.

At this time, thousands of mites, representing more than 30 species, were collected from areas where they had no opportunity to become infected and they were kept in the laboratory for later infection experiments when eggs of rabbit cestodes would be available in large numbers. Meanwhile, many of the mites laid eggs, and young, laboratory-raised specimens were used in later experiments. During the autumn and winter, while rabbits and hares were plentiful in the markets, tapeworms and their eggs could be obtained at any time. Eggs of two species of cestodes, *Cittotaenia ctenoides* and *C. denticulata,* were fed to the mites taken from uninfected areas and to their laboratory-raised progeny. Successive stages in the development, from the onchosphere to the cysticercoid, were obtained in numbers. Development of *C. ctenoides* was observed in eleven species of mites and cysticercoids were recovered from seven of them. About ten weeks are required for the larvae to complete their development in the mite. Development of *C. denticulata* occurred in six species of mites and fully formed cysticercoids were recovered from *Scutovertex minutus.* The life cycle had thus been completed under controlled, experimental conditions. Successive stages were removed from the body cavities of the mites after they had been experimentally infected, and experimental demonstration has proved that the cysticercoids become sexually mature tapeworms in the intestine of domestic rabbits. Successive stages in the development of the worms in the final host were removed from naturally infected wild rabbits. A detailed report (Stunkard, 1941), described the experiments and results of the study on the life cycle of rabbit cestodes.

The discovery of the life cycles of the tapeworms of sheep and of rabbits was supplemented by observations on yet another species. Although the anoplocephaline cestodes are found chiefly in the lower herbivorous animals, species have long been known to occur also in monkeys. Professor Studer of Bern, Switzerland, in 1879, found two of these worms in the intestine of a chimpanzee which had died in the zoological park at Lyon. One of the worms was studied by Professor Blanchard of the medical faculty of Paris, who (1891) named the species *Bertia studeri.* The name *Bertia* was preoccupied, so Stiles and Hassall (1902) renamed it *Bertiella.* When Blanchard described *B. studeri* from the chimpanzee, he declared that it was a potential human parasite and, twenty-two years later, he (1913) was the first to record it from man. The specimen was recovered from an eight-year old child at Port Louis, Mauritius. Other cases were reported from India and Africa, and Garcia (1935) described the parasite from a child, a monkey and a dog in the Philippine Islands. According to Joyeux and Baer (1929), *Bertiella studeri* is a common parasite of monkeys in Asia and Africa and these animals probably constitute the source of the infrequent human infections. Cram (1928) and Cameron (1929) reported human infections in the West Indies and it is clear that this tapeworm is an established human parasite in both the eastern and western hemispheres. There was no information concerning the life cycle and transmission of the monkey-human species until 1939, when I presented a report at the Third International Congress for Microbiology. The abstract of that report appeared in the proceedings of the Congress, and a longer account in the American Journal of Tropical Medicine (Stunkard, 1940). The study was made at the Institut in Hamburg. One of the monkeys in the animal house, a specimen of *Macacus rhesus,* originally from India, was passing tapeworm segments in the stools. Examination of the material showed that the proglottids and eggs belonged to an anoplocephaline species, probably *Bertiella.* Since free-living mites serve as intermediate hosts of sheep and rabbit tapeworms, it appeared probable that the monkey-human species had a similar life cycle. Accordingly, several hundred free-living mites, belonging to 24 different species, were collected from locations in the vicinity of Hamburg where they would not have been exposed to infection by tapeworms. Thousands of eggs were taken from the gravid proglottids passed by the monkey and these eggs were fed to the mites. Dissections of mites were made at frequent intervals and showed that the mites ate the eggs, that the larvae emerged and migrated to the body cavities, where, in certain species, they followed a course of development practically identical with that of the sheep and rabbit cestodes. Fully formed cysticercoids were recovered from two species of mites and partial development was observed in others. The monkey was sacrificed and three specimens of *B. studeri* were taken from the intestine. It is clear, therefore, that the monkey-human species, like those of sheep and rabbits, utilizes free-living mites as intermediate hosts.

The results so far obtained have disclosed the life cycles of certain of the anoplocephaline cestodes of sheep, rabbits and monkeys, viz., *Moniezia expansa, Cittotaenia ctenoides, C. denticulata* and *Bertiella studeri.* Representatives of three genera have a common developmental pattern and all employ oribatid mites as intermediate hosts. It is probable, therefore, that related forms have a similar life history and a beginning has been made in the solution of the problems "How do the tapeworms of herbivorous animals complete their life cycles?" Only further studies can provide the final answer.

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WILLIAM A. RITCHIE, Rochester Museum of Arts and Sciences: *The Pre-Iroquoian Occupations of New York State.* (A. Cressy Morrison Prize Winner, 1943.)

The purpose of this work is to afford systematic expression to existing knowledge of aboriginal cultures in New York State, prior to the appearance, or at least to the dominion, of the Iroquois tribes, whose vanguard, it is believed, appeared in the wooded hills of our southeastern counties early in the fifteenth century of the Christian era. The data embodied in the correlation have been garnered over a period of many years by numerous individuals, lay and professional, to whom full credit is given in the appropriate pages. Principally, however, the reconstruction rests upon the investigations of the Rochester Museum of Arts and Sciences, conducted since 1924 when Dr. Arthur C. Parker became its director and the writer its archaeologist. Much of the material herein described has not previously been published, while that appearing in monographic studies has been re-evaluated and revised in the light of the whole before incorporation.

The surveys and excavations of the Rochester Museum have covered virtually the entire state except for its eastern portion; the knowledge so gained being supplemented by analyses of collections, private and institutional, situated throughout New York and its environs. While the present conception of culture configuration and sequence is not in any sense a fully formed imago, it is believed to be essentially sound, awaiting only further persistent efforts to develop and perfect its parts.

ARNOLD B. GROBMAN, Biological Laboratories, University of Rochester: *The Distribution of the Salamanders of the Genus Plethodon in Eastern United States and Canada.* (A. Cressy Morrison Prize Winner, 1943.)

Less than two decades have passed since the appearance of Dr. Dunn's (1926) excellent monograph on "The Salamanders of the Family Plethodontidae." Its inclusion of locality spot maps, showing the distribution of each form considered and a thorough discussion of their relationships, would indicate a certain degree of temerity on the part of the present writer in presenting an account of the distribution of *Plethodon,* one of the genera of this family. There is justification for this presentation, however. Much additional material has accumulated in museum collections since 1926. In this paper, for example, there are sixteen forms recognized in the eastern Plethodons where Dunn had but eight. One entire species group (Welleri), containing three species (*richmondi, nettingi,* and *welleri*), was not known when Dunn's book was published. Specimens have become available from regions that were comparatively inaccessible only a few years ago due, largely, to the expansion of the public highway system. Further, an attempt is made here to elucidate the facts of distribution by the consideration of applicable information from the recent literature of the cognate fields of glacial geology, physiography, phytogeography, genetics, and experimental embryology. Finally, it should be pointed out, that much new information about the genus under consideration has been made available since 1926 by Bailey, Bishop, Brimley, Dunn, Green, Heinze, King, Mittleman, Netting, Walker, and others.

This report is designed to be a study of geographic distribution. Those who are not familiar with the salamanders under discussion may read it with most profit if a copy of Dunn's book is at hand. It is considered beyond the scope of the present work to include morphological descriptions and an analysis of systematic characters; these are to be found in Dunn's book. However, where new forms are described, and where new groupings are proposed, a consideration of their systematic characters has been incorporated.

The original plan of this study was to survey the distribution of all eastern forms of the family Plethodontidae and this still remains the over-all plan. Because of the uncertainty connected with existing conditions, however, it was thought best to present at this time such material as has been compiled. Other sections, on the remaining genera, may be looked for at varying intervals in the future. A treatment of *Desmognathus* and *Leurognathus* is now in preparation.

The present paper deals with the genus *Plethodon* in eastern United States and Canada. Certain western species of *Plethodon* are not considered to be within the scope of this report. These, listed in the chronological order of their descriptions are: *P. vehiculum* (Cooper), *P. vandykei* Van Denburgh, *P. elongatus* Van Denburgh, *P. dunni* Bishop, *P. idahoensis* Slater and Slipp, and *P. hardii* Taylor.

The eastern members of the genus may be divided into two distinct groups—one including the larger Plethodons with fewer costal grooves, and the other, the smaller Plethodons with more costal grooves. These two groups differ sufficiently from each other to be considered subgenera or even distinct genera. Allocations of this nature cannot be made, however, until the western members of *Plethodon* have been brought into the scheme; here the two groups will be referred to as the Large Plethodons and the Small Plethodons. The Large Plethodons contain two species groups, Metcalfi and Glutinosus, and the Small Plethodons, likewise, are comprised of two species groups, Cinereus and Welleri.

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JOINT MEETING

SECTIONS OF BIOLOGY AND PSYCHOLOGY[1]

JANUARY 17, 1944

PROFESSOR EDWIN G. CONKLIN, American Philosophical Society, Philadelphia, Pa.: *Ends as Well as Means in Life and Evolution.*

INTRODUCTION

I am proud to be an honorary member of The New York Academy of Sciences, but I have not heretofore been able to attend any of its meetings, although I read with interest and profit its publications. Once before I was prevented by sickness from attending an Annual Meeting, at which I was scheduled to speak. Therefore, when the Chairman of the Section on Psychology invited me to speak on this occasion, I felt that I could not refuse, although I am no psychologist. I am by courtesy a biologist, but my field of research is embryology and cytology. However, these subjects have broad bearings on all biology and psychology and they even afford a "take off" into realms of philosophy. I hope this *apologia* may explain my presence at this meeting, my choice of subject, and my limitations in dealing with it.

I fear that this topic, "Ends as Well as Means in Life and Evolution," is out of date, if not out of place, in a time and center so noted

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[1] No meeting was held in January by the Section of Geology and Mineralogy.

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for its work in experimental biology and psychology, for I appear as the voice of one crying in the wilderness of analytic and mechanistic science, not to proclaim a New Dispensation, as did John the Baptist, but to recall biologists to some of the problems of the older naturalists. The extreme specialization of science has tended to obscure the larger aspects of Nature, as a whole. It has been said that modern science consists in knowing more and more of less and less. Certainly it is concerned largely with analysis and always more analysis. Living things are analyzed into organs, tissues, cells, chromosomes, genes, and their functions into tropisms, reflexes and forced movements, while the synthesis of all of these elements into the broader aspects of the organism and its relation to environment are too much neglected. Of course, analysis is necessary, but so also is synthesis if we are to see organisms as living beings. We lose sight of these larger aspects of life in the process of analysis, unless we reverse this procedure from time to time and consider organisms and environment synthetically.

Life has been compared to a beautiful tapestry, woven in intricate design of many threads and colors. By means of physics, chemistry, physiology, anatomy, embryology and genetics we unravel this texture, separate its constitutent threads and colors, but lose the pattern as a whole. These analytical sciences have enormously increased our knowledge of life's constituent elements and processes, but the pattern of the tapestry is usually neglected or ignored.

We are often assured by analytical scientists that genuine science concerns itself only with means and mechanisms. It asks only the questions What, How, When, but never Why. But the human mind insists on asking Why, for this is the most important of all questions in dealing with human experiences and relations and it is the most intriguing and baffling of all questions regarding nature. Aristotle, the greatest naturalist of antiquity, recognized four classes of inquiries regarding the causes of phenomena: (1) material causes, (2) efficient causes, (3) formal causes, and (4) final causes. Francis Bacon said that the first and second of these belonged to physics, the third and fourth to metaphysics. Concerning final causes, he said that they were like the Vestal Virgins, sacred but barren. Huxley compared final causes to the Hetaerae with whom philosophers amuse themselves. But no philosopher, scientist, or average human being can avoid asking the question Why, or fail to feel that the end, goal, meaning or purpose of any phenomenon in nature is the most significant inquiry that can be made about it. Aristotle, "the master of those who know," was great in every field of knowledge, but greatest of all in zoology. In his "De Partibus Animalium" he maintained that the essence of a living animal is found not in *what* it is, or *how* it acts, but *why* it is as it is and acts as it does. This question *why* is a hard one to answer, perhaps impossible to answer fully or satisfactorily, but it is one that cannot be wholly disregarded, and, by facing it frankly, we may be saved from much shallow thinking, cocksureness and narrowmindedness. Let us take a long, a broad view of the living world, see something of the pattern of the tapestry of life which has been woven throughout the ages and face, frankly, the questions *what, how* and *why,* problems not only of ways and means but also of ends and meanings.

ENDS AND VALUES IN LIVING PHENOMENA

Herbert Spencer said that life consists in continual adjustment of internal conditions to external ones. This adjustment of living things to their environment is more than physical and chemical equilibrium; it is more like the conscious adjustments that human beings make to hunger, thirst, cold, etc., and the unconscious adjustments that animals make to pleasant or unpleasant conditions. Upon such adjustments the continuance of life depends; when it fails, life ceases.

So far as the astronomer or physicist or chemist knows, it makes no difference to a star, or atom, or molecule, whether it continues to exist as such or not; but the biologist sees abundant evidence that it makes a great difference to a living thing whether it continues to live or not. Innumerable structures and functions, adjustments and regulations are found in living things to enable them to continue to live. The trend, or drive, or struggle for existence is one of the most universal of all living phenomena. Continuity of life is the great *end* of living; survival its greatest *good*; extinction its greatest *evil.* The moment we pass from the lifeless to the living world, we enter a world of ends and values. And, until physics and chemistry can deal effectively with these phenomena of life, biology must remain an autonomous science.

But, while adjustment or adaptability are characteristic of all life, special adaptations or fitness to special conditions are usual but not universal, and they are relatively but not absolutely perfect. Such adaptations are seen in many structures, functions and relations, but not in all. Sooner or later these adaptations always fail and death results. The occasional lack of adaptations to meet specific conditions and the general difficulty of explaining them mechanistically have led some extreme mechanists to deny the existence of adaptations and thus to explain them by explaining them away. At the meeting of the American Society of Naturalists in 1912, I presided over a symposium on adaptation, in which one of the speakers denied that there were any such things as adaptations. Another speaker expressed his amazement at such an assertion; he said that he had always supposed that fishes were fitted to live in water, birds to fly, moles to burrow, that eyes were for the purpose (or end) of seeing, ears for hearing, muscles for motion, and so on through hundreds and thousands of fitnesses for particular functions or conditions.

Let me begin by pointing out the fact that purposive phenomena are usually wholly distinct from conscious purpose. Indeed, even in man, consciousness occupies a relatively small place in our lives. The larger part of our total existence, from conception to death, is passed in a condition of unconsciousness; and yet the structures and functions of the germ cells from which we came, their maturation and fertilization, the cell and nuclear divisions which give rise to tissues, organs and systems, the complicated chemical and physical phenomena involved in digestion and assimilation, in respiration and excretion, in irritability, sensitivity and regulation, all are purposive in that they are directed to certain ends, and yet they are wholly unconscious. Evidently usefulness, fitness and purposiveness are not necessarily nor usually accompanied by conscious purpose. With this brief introduction to a vast theme and this explanation of some of the terms and phrases which must be used, we will consider, all too briefly, some of the multitude of ends and fitnesses of animals and plants.

1. Let me briefly classify some of the outstanding fitnesses of living things, and pardon a zoologist for mentioning, chiefly, the fitness of animals rather than those of plants. And let us consider in the first place racial or inherited adaptations which have arisen in the course of evolution,—according to generally accepted opinion. Here are found most of the striking fitnesses of organisms, such as the adaptation of animals and plants for particular media or habitats, such as the fitness of fish and seals and whales for life in water; the fitness of birds for flying, where lightness and strength of feathers, wings, muscles and bones have not been excelled by any materials in airplane construction; the remarkable adaptations of animals and plants to extreme changes in temperature, humidity, atmospheric or submarine pressures. There seems to be

almost no place on earth into which some organisms have not penetrated and become at home.

2. Another class of environmental adaptations are found in the means of offense or defense against other living enemies, such as strength or speed, armor, horns, tusks, stings, fangs, stink gas, colors, camouflage. Almost every means of offense or defense known to man is found in different animal species.

3. Still another class of interorganismal adaptations is found in the interrelations of flowers and insects, which Pastor Konrad Sprengel first explored in 1793 in his "Neue entdeckte Geheimnis der Natur." Also in the adaptive relationship between parasites and their hosts, where the ingenuity of omniscient fiends could scarcely exceed the infernal ingenuity of the malarial parasite, for example, in undergoing change of form, host and particular location, by which it is enabled to gain admission to the blood of man or other animals. Finally, in this class of interorganismal adaptations, consider all the marvellous adaptations of sex, ranging all the way from the peculiar structures and functions of spermatozoa and ova to the behavior and psychology of males and females in courtship, mating and care of young. Nothing in all nature exceeds in complexity and nicety of fitness such adaptations of sex.

All of these classes of adaptations, with thousands of other examples, are racial or inherited fitnesses; and the problem of their origin is the great problem of evolution.

4. Another class of adaptations are not racial or inherited but are acquired by animals and plants in the course of their individual lives. They consist in beneficial responses to stimuli and are therefore problems of acquired habits or behavior. Among these, one may note the following beneficial responses to particular stimuli: Increased light of short wave length leads to increased pigmentation of the skin, which protects the underlying tissues from injury. Increased use of a muscle leads to its increased size and strength. Increased cold leads to increased thickness of hair in mammals, to increased activity and feeding on the part of hibernating bears. Bees exposed to cold weather gather into a dormant cluster in the middle of the hive, where they keep one another warm. If those at the surface get too cold they become active and burrow into the middle of the cluster; if the temperature throughout the cluster falls to about 57° F. they all become active, raise their temperature by muscular work, take in food, and when their temperature has risen to about 90° F. they again become inactive or dormant.

Gradual increase of heat or cold leads to an acclimatization to temperatures that would be fatal at once if they came suddenly. A similar tolerance to poisons may be acquired, if only the poisons are administered in gradually increased doses. Bacterial poisons, such as diphtheria toxin, are neutralized by the formation of antitoxins, if only the body has time to form these in sufficient quantity, and thereafter the individual has an acquired immunity to that particular toxin. Finally, in this class of acquired adaptations may be mentioned the great number of adaptations to injuries, ranging all the way from wound-healing to the regeneration of lost limbs as in lobsters and salamanders.

Nothing in the whole world is more wonderful than this ability of organisms to overcome adverse conditions, to neutralize violent poisons, to restore lost parts. Many of these conditions could never have been met before by their ancestors, and therefore these animals could not have inherited this specific fitness. Thus guinea pigs, whose ancestors came from South America, never had opportunity to become adapted to the poison of cobras from India and yet, if the cobra venom is administered in graduated doses, the guinea pigs acquire a tolerance to the venom. Injuries such as could never have happened except under expert surgical operations may be followed by regeneration; thus, if the lens of the eye of a newt is removed it is often completely regenerated. Newts may have lost whole eyes or heads in the past, but certainly never before have they had the lens of the eye removed.

CAUSES OF ADAPTATION

How have such adaptations arisen, whether racial or individual, inherited or acquired? This is the greatest problem of life and evolution. Innumerable attempts have been made to solve this problem by philosophers and scientists. These attempted solutions may be classified as supernatural, vitalistic, or mechanistic.

1. Supernatural causes presuppose an Intelligence and Will outside of Nature, a *deus ex machina*. This is the old "Argument from Design" for the existence of God. It reached its climax in the "Natural Theology" of William Paley (1802) and in the eight Bridgewater Treatises on "The Power, Wisdom and Goodness of God as manifested in the Creation," which were published under the auspices of the Royal Society from 1836 to 1840. It broke down because of the fact of conflicting designs, as in the case of the fitness of the host to repel the designed attack of the parasite, or the conflicting designs in the opposing

fitnesses of the prey and the predator. It also seemed inconsistent with divine design that there are so many imperfect, rudimentary or incomplete fitnesses; a perfect intelligence should have produced more perfect results. But the compelling evidence against supernatural design came from the growing evidence in favor of natural evolution and from increasing confidence that natural phenomena have natural causes. While the Bridgewater Treatises were being prepared and published, Charles Darwin was on his famous "Voyage of the Beagle," collecting the material and developing the ideas that 23 years later appeared in "The Origin of Species by Means of Natural Selection." What Huxley called "the frivolities of supernatural teleology" are part and parcel of the frivolities of special creation. Few, if any, scientists now admit the special creation of species or the special creation of adaptations.

2. Vitalistic hypotheses assume some sort of intelligence, or will, or psychic principle in organisms themselves that act as guiding or directing causes of adaptation—a sort of *deus in machina.* Among these hypotheses are the "perfecting principle" of Aristotle and Nägeli, the "indwelling soul" of Plato and Bruno, the "active teleological principle" of Kant, the "will" of Schopenhauer, "desire, need, and appetency" of Erasmus Darwin and Lamarck, "unconscious purpose" of Hartmann, "vital force" of Bunge, Wolff and Virchow, "élan vital" of Bergson, "entelechy" of Driesch, "psychism" of Pauly, Boveri and Spemann. The chief objection to these hypotheses is that they are mere names, ghosts without substance, not open to experiment or analysis. Therefore, they tend to prevent further inquiry and are a hindrance to research, unless they are recognized as signposts pointing to the unexplored.

3. Mechanistic explanations of fitness are based largely on Charles Darwin's principle of Natural Selection or the elimination of the unfit and the survival of the fit. If all organisms vary in relative fitness, and if those that are less fit are eliminated, while the more fit survive, there will be a continual drive toward fitness. There is no doubt that this is true and that this elimination of the unfit is much more extensive in germ cells and embryos, which we usually do not see, than in adults. This simple principle of natural selection does explain, as nothing else does, inherited adaptations of all kinds, though of course it does not explain the origin of the useful mutations that are selected.

But classical Darwinism does not explain individually acquired adaptations where useful responses are made by animals and plants to conditions and crises which they or their ancestors never experienced before. In such cases there is no elimination of unfit individuals; the same animal or plant continues to survive and gradually acquires the ability to make the useful response. In many cases, we know that this is done by a process of trial and error and, finally, trial and success. This is an extension of the principle of the elimination of the unfit from persons to reactions, and at first sight it seems to be a purely mechanistic explanation of acquired fitness. But it is not purely mechanistic, for it is based upon the ability of living things to distinguish between the satisfactory and the unsatisfactory, and to follow after the one and to avoid the other. This capacity must be regarded as a fundamental property of living things, which cannot at present be reduced to simpler terms. Thus, we endow life at the start with the very properties which we seek to explain: namely, the capacity to distinguish and select, which is the beginning of wisdom in all living things.

4. Between vitalism and mechanism there is a middle ground which may be called "Organizationism" or "Emergence," which holds that life, differential sensitivity and reactivity, fitness and psychic phenomena, are results of increasing organization, these new properties "emerging," as it were, by a process of creative synthesis. This organizational theory was introduced by William E. Ritter in 1919, and later by C. Lloyd Morgan in 1923 under the name of "emergence." It has been approved by many leading scientists, including W. M. Wheeler, H. S. Jennings, W. E. Agar, Lovall Evans, A. V. Hill, J. S. Huxley, E. S. Russell and D. M. S. Watson. But emergence of new qualities, while a fact, does not offer a purely mechanistic explanation of life, evolution and fitness, for underlying all of these there seems to be required some directive or teleological principle. While this teleology may seem to many to go back to old philosophical speculations that border on mysticism, the names of such experimental biologists as have been mentioned, and of such outstanding physicists and chemists as Einstein, Planck, Niels Bohr, Otto Loewi and Lawrence J. Henderson, should warn us that there may be good grounds for holding that mechanism and teleology are complementary views of nature, neither excluding the other. For myself, I may say that I have no sympathy with hypotheses of "vital forces" or "vital principles" that are wholly unrelated to other forces and principles of nature; but with the idea that organisms are endowed with differential sensitivity and useful tropisms and that there is some genetic relationship between the fitness of behavior

and that of evolution, between individually acquired adaptations and inherited or racial ones, I have a great deal of sympathy.

Postponing, for the time, the further consideration of the origin or evolution of inherited fitness, let us look at the adaptive behavior of animals, from the highest to the lowest, rather than in the reverse order. It is the practice of embryologists to look at development from both ends of the process: that is, to study the development of the egg through its various stages until it gives rise to developed organs, and to work backward from organs to their earliest beginnings in the embryo and egg. Only in this way is it possible to trace the origin of many developed characteristics, the beginnings of which would be overlooked in greatly simplified early stages. Of course, the evolutionist follows this same practice, for the beginnings of fully evolved structures and functions and adaptations would be overlooked unless they were traced backward from the later to the earlier stages in evolution.

We are generally cautioned to be on guard against the dangers of anthropomorphism: that is, of considering animals from the standpoint of man, and yet, at the same time, we are assured that man is only a later stage of development of the animal series. It should be possible to find the early beginnings of human behavior and acquired adaptations in the behavior of animals below men. How do human beings acquire habits of useful behavior? By a process of trying all things and holding fast to that which is good, as St. Paul expressed it. This process has long been called a method of "trial and error" and, finally, trial and success. I do not know who first used the phrase "trial and error," but I find it referred to, as if in common use, by Huxley in his discussion of teleology in his article, "Criticisms of the Origin of Species," which is dated 1864 (Collected Essays, Vol. II, p. 84). Later studies of animal behavior have shown that cats learn to let themselves out of cages and that horses learn to open gates or doors and that higher animals in general learn to do useful things by this process of trial and error. In 1904, and again in 1906, Jennings found that lower organisms, especially *Paramecium* and *Amoeba,* reach adaptive or useful ends by this same process of trial and error. For example, *Paramecia* in a trough of water, which is hot at one end and cold at the other, swim on random courses until they come into water which is hot or cold, when they back, turn into a new course and go ahead until they again run into hot or cold water, when the avoiding action is repeated, with the final result that they are found in water of medium temperature. This useful or adaptive result has been attained by a multitude of random movements, elimination of those that are injurious or unsatisfactory and persistence in those that are beneficial or satisfactory. *Paramecium* differentiates or distinguishes between the satisfactory and the unsatisfactory, or what E. C. Tolman (1941) has recently called "satiations" and "sufferances," and this introduces a psychic factor, which strict mechanists, such as Jacques Loeb, repudiate; and yet, as evolutionists, we should be ready to trace the adaptive and psychic processes of higher animals to their beginnings in lower forms. To attribute such adaptive results to tropisms merely puts back into tropisms this problem of adaptation. Of course, I am not forgetting that development and evolution are in the main epigenetic processes by which the more complicated end stages are built upon the less complicated earlier ones, but I also refuse to forget that these earlier stages are also complex, that the egg or the *Paramecium* are complex organisms and that development is endogenetic as well as epigenetic. Both epigenesis and endogenesis are involved in all development and evolution.

Apparently the adaptive responses of plants as well as animals grow out of differential sensitivity to beneficial or injurious stimuli with positive reactions to the former and negative ones to the latter. The botanist Macfarlane (1918) maintained that "Plants have the capacity of growing or moving toward the environment most satisfying." One sees this in the positive phototropism of shoots and the negative phototropism of roots: e.g., in the growth of shoots of potatoes for long distances toward cellar windows, or in the growth of roots of trees for many feet toward water drains. Such beneficial behavior comes from growth in various directions, elimination of the non-beneficial and persistence in the beneficial responses. The plant differentiates and selects between the two.

The *elements* of our own behavior are found in all organisms. Living things generally act as if some stimuli or environments are satisfactory and are responded to positively, while others are unsatisfactory and are responded to negatively. Protoplasm does not respond as a stone or an inert substance. In the words of Zur Strassen (1915), it is full of "organized seekings," and by processes of trial and error these lead to useful or fit results. You may call this anthropomorphism when applied to *Amoeba* or amoeba-morphism when applied to man, but it is something psychic out of which mind and purpose develop. When Loeb had reduced animal behavior to tropism and forced movements he had not eliminated differential sensitivity and reactivity, which are the elements out of which satisfaction and dissatisfaction, discrimination and selection, intelligence and purpose develop.

Acquired adaptations or useful behavior develop by a process of trial and error, which involves the capacity to distinguish and select between the satisfactory and the unsatisfactory. Is it possible that a similar process is involved in inherited adaptations or the evolution of the general fitness of organisms in nature? Charles Darwin's process of natural selection seems to be a parallel process. Here we have an overproduction of mutations, good, bad and indifferent, while the environment weeds out the bad and leaves only the good or indifferent: that is, there is survival of the fit, not necessarily the fittest. But in classical Darwinism it seems that the environment eliminates or selects, while the organism is passive in this process, whereas in adaptive behavior it is the organism that selects or rejects. Perhaps further analysis would show that this difference is not so fundamental as at first appears, for even in natural selection it is the failure of the organism to meet conditions of the environment that leads to its elimination. Another distinction between the natural selection of the fit and the trial and error process in behavior is that, in the former, unfit individuals or groups of individuals are eliminated, whereas, in the latter, only unfit responses or reactions are eliminated, while the individual persists. But with these exceptions there is a fundamental resemblance between natural selection and trial and error.

Lamarckism seems to solve very beautifully the problem of the evolution of adaptations by assuming that adaptive behavior becomes hereditary, but unfortunately there is no satisfactory evidence in favor of the inheritance of acquired adaptations, no evidence that the experience of an animal or plant in meeting and overcoming adverse conditions can be pushed back into the egg or sperm, or their chromosomes and genes, so as to reappear in the next generation as an inherited adaptation. It is too bad that Lamarckism is not true, for it would afford a ready explanation of inherited adaptations.

We seem compelled then to fall back upon Darwinism, together with all the aids furnished by Mendelism, mutation, genes and statistical studies of the survival rates of certain genotypes, in order to account for the origin of inherited adaptations. And yet, to me, this seems an incomplete and even an improbable solution of this great problem. When one considers, for example, all the adaptations and coadaptations that are found in the eyes of vertebrates—the remarkable fitness of the retina with its rods and cones for receiving and transmitting the stimuli of light of varying intensities and wave lengths; the beautiful dioptric apparatus of transparent cornea, lens and humors; the elastic lens with the ciliary muscles for focusing the light coming from near or far objects; the iris with its intrinsic muscles for controlling the amount of light admitted; the intrinsic nerve supply and blood vessels; the accessory parts for the protection of this delicate apparatus, the eyeball with its tough outer coat, the eye socket and bony orbit, eyelids, eyelashes, eyebrows, lachrymal glands and ducts, etc.,—when one considers such an assembly of remarkable adaptations and coadaptations, he cannot fail to ask whether all these have been produced by purely fortuitous mutations, followed by the elimination of those individuals that were not well adapted. Do we not load upon chance mutation an impossible burden in requiring it to provide all the functions and structures of such remarkable fitness? Surely there must be other factors than those now generally recognized in the origin of such wonderful adaptations. It is no wonder that Darwin said that he never thought of attempting to explain the origin of the eye without a shudder.

I have no theory to propose to explain the origin of organic fitness and human purpose other than the endowment of living beings with differential sensitivity and reactivity, tropisms, organic memory, trial and error behavior, leading in higher animals to intelligence and purpose. I do not doubt the reality of the mechanistic principles of physics and chemistry and natural selection in the evolution of organisms with all their adaptations. But with crude mechanism that finds everything the result of accident and chance I have no more sympathy than with transcendental vitalism. Those who say there are no ends, values, purposes in the living world may understand atoms, molecules and even genes, but they do not understand organisms. They see the elements of which life is composed, but they fail to see the pattern and beauty of the entire tapestry of life.

SECTION OF ANTHROPOLOGY

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MR. GREGORY BATESON, Associate in International Administration, Columbia University, New York, N. Y.: *Pidgin English and Cross-Cultural Communication.*

One of the most pressing problems, in a world which is becoming increasingly close-knit, is that of mutual understanding between peoples with basically different cultures, different value systems and different emotional attitudes to life. In this field, language is one of our most essential tools and whenever we have a problem in cross cultural communication between peoples of different language our thoughts turn naturally to the possibility of using some easy *lingua franca*—Basic English, Esperanto or Pidgin.

In this paper, I want to discuss the Pidgin English of the Mandated Territory of New Guinea, a *lingua franca* which was not consciously constructed like Basic English or Esperanto but which developed naturally as a medium of communication between whites and natives in the New Guinea environment.

Historically, this particular Pidgin differs from most of the trade languages of the world in that it has become stabilized. Usually, these simplified languages are steadily enriched, either from the white side or from the native, because one side at least feels the trade language to be a corruption of its own and therefore tries to "correct" it. A little progress of this sort probably occurred in New Guinea, because housewives in Rabaul spoke very poor Pidgin and therefore introduced English words in talking to their cook-boys, and still more progress of the same sort may result from contact between Anglo-American troops and New Guinea natives. In general, however, when I met with it, Pidgin was a stable language and had been stabilized by the Germans. Pidgin English grew up in New Guinea before the area became a German colony in the 1880's, and the Germans, therefore, arrived to find that they must learn a language which was not theirs. They were not tempted, as the English are, to make the language more like their own, but simply learned it as a foreign language. They learned it and thereby contributed nothing to it, except stability. The whole vocabulary of Pidgin—about a thousand words—contains less than a dozen words which can clearly be recognized as of German origin.

Another factor which has contributed to the stabilization of this Pidgin is the multiplicity of the native languages in the New Guinea area. Every five or ten miles along the road the traveler meets a new language, and there are something like a thousand languages in New Guinea. When a native leaves his village and goes to work as an indentured laborer in the white man's plantations or gold mines, he finds, on the labor line, a mixture of other natives from as many as twenty linguistic areas, and Pidgin therefore becomes naturally the *lingua franca* for communication between natives of different linguistic origin.

Pidgin may be described as a hybrid between English and the Melanesian family of languages which predominate in the coastal areas of New Guinea which were first colonized (the Gazelle Peninsula, New Ireland, Admiralty Islands, etc.). But this hybridization has not been on random lines. The language is not a mere mixture of English and Melanesian characteristics. Rather, it is a systematic fitting of English vocabulary to Melanesian grammar and syntax. Between seventy and eighty per cent of the vocabulary is of English origin, the remainder being made up of words from mixed European and native sources. In a few cases, a single word may combine both English and native roots. The word *liklik,* meaning "small," for example, is such a hybrid between the English *little* and *ikilik,* the word for "small" in the language of Rabaul. The moment, however, we leave the details of vocabulary and consider the system of grammatical structure, we find pure Melanesian forms, even down to such details as duals, inclusive and exclusive pronouns, etc.

The question which we have to consider is whether such a hybrid language does, in fact, serve to communicate the culturally patterned thoughts of the white man to the native. Inversely, does it serve to convey the culturally patterned thoughts of the native to the white man? Are the two peoples brought into closer communion by Pidgin English?

This question clearly transcends all the details of grammar and vocabulary, which are the ordinary stuff for linguistic study. It asks, in fact, whether the ideas and emotions which normally occur in Pidgin conversation are in any sense a mixture of the white man's ideas and emotions with the ideas and emotions charactistic of native life. And the answer appears to be a flat negative.

I do not want to give the impression that Pidgin English is a poor language. Its vocabulary is big enough, its tones of voice and emphasis are rich and varied, and when one speaks Pidgin one has no feeling of frustration. My own feeling—and that of most white men—is of handling a rich and flexible language, and the turns of phrase which must be coined in order to convey some word not in the vocabulary, increasing rather than decreasing the feeling of liveliness in the language.

The truth is that Pidgin, in spite of its hybrid origin, constitutes something new. The language and its tones of voice and the things that are said in it are a rudimentary third culture, neither native nor white, and within the conventions of this third culture the white man and the native can meet happily, though the culture is germane to neither of them. Each may have in the back of his mind a whole mass of thoughts and values and interpretations of the world which are specifically his. The white man has his notions of competition, sacrifice, fair play, justice, checks and balances, democracy, discipline and so on, and the native correspondingly may have in the back of his mind his own interpretations of life, his feelings for symmetry in human relations, his notions of the sacred and the unclean, of gift exchanges, and the validation of social events. But neither the white man's philosophy of life, nor that of the native, crops up in the neutral and special fields of Pidgin English conversation. It is not that democracy and private enterprise could not be described in Pidgin, it is just that, in fact, they are not. The language is quite rich enough to convey anything that the speaker clearly understands, but it is not close enough to English to convey something which the speaker has not himself analyzed. There is no single counter in Pidgin which could be substituted for any one of the highly valued counters and symbols of our political, religious and social life. The white man, when he enters the world of Pidgin English, unless he is willing to dissect his own values down to their simplest terms, must therefore leave these values behind, so far as conversation is concerned, and must deal in the specific values—equally vague but differently organized—of the Pidgin world. Similarly, the native must leave behind his own special values and ideas and embark on a new world.

The emotional content of the Pidgin English culture is difficult to define—it is always difficult to define the ethos of a system of human behavior. The normal techniques for such an analysis are based on the process of acculturation of children. When we study a native tribe we collect the system of stylized behavior and relate this back to the way in which children are brought up to be members of their tribe. Out of the counterpoint between adult behavior and parent-child relations, we build up a coherent picture of the emotions and ideas which are systematically bound up together—partly explicit and partly implicit—in the culture. For Pidgin English, this has never been done. There is nothing equivalent to the parent-child relationship, and the induction of the native boy and the newly arrived white man into the Pidgin English culture follows no standardized lines. A genetic study is scarcely possible.

The ethos of Pidgin English is, however, shaped by certain negative features. It is, like the rudimentary culture which develops between passengers on a ship, a rather gay, rather irresponsible and meaningless world. Both white man and native have put aside many of the fundamentals of life. The indentured native has left his kin behind in the village and is embarked on an adventure which he knows will one day end. When he goes home, three or four years later, he will be proud that he, too, has "worked"; but, for the time being, he is suspended in an interim period of life whose length is fixed by his contract of service, as is the length of a voyage or a prison sentence. The world on which he enters is almost exclusively male; it is happy, cooperative and simple. Like a soldier entering the army, he must learn to obey a daily routine; his food and sleep are automatically included in this routine; and the complexities and responsibilities of his normal background are in abeyance. Between the white man and the native there is continual emotional give and take resembling in some ways the sort of emotional bond which—whether it was stable or not and whether it was desirable or not—was at any rate a clearly patterned relationship between white men and Negroes in the American South.

The Pidgin world is characterized, too, by overtly recognized caste structure. The line between white and native is recognized by both sides and is regarded by both sides as based on superiority and inferiority. Most of the native cultures of New Guinea are almost without heirarchical structure; chieftanship is comparatively rare; and there is little to correspond to the elaborate status systems of Indonesia and Polynesia. The native, therefore, when he enters the Pidgin world, must take on patterns which are foreign to him,—he must act out the rôle of inferiority vis-à-vis the white man, and if he becomes a boss-boy or a member of the native constabulary he must act out the role of superiority vis-à-vis the other natives. He learns to do this without much difficulty, but the presence of this hierarchical theme in the Pidgin culture means that the whole system of Pidgin life is foreign to him. It is a culture put on in late adolescence, not one for which he was prepared in early infancy.

In sum, it appears that Pidgin is not a means whereby the white man communicates white thoughts, nor is it a means by which the native communicates native thoughts. It is another world in which white man and native meet. In this other world they feel that they communicate fully and richly, but the matter of their communication is not closely related to the system of life in which either of them grew up.

This diagnosis, which recognizes Pidgin English as, on the whole, a barrier between white man and native, raises a number of problems of long-time policy in regard to the future development of any area within which a *lingua franca* has become stabilized. In New Guinea, the multiplicity of native languages precludes any possibility that any one of these might become a *lingua franca,* and the stability and richness of Pidgin will make it exceedingly difficult to introduce English. Human beings are lazy and are unlikely to learn a difficult language when an easy one is at hand for casual communication. It is, however, possible that Pidgin English might be enriched, both from the native side and from the white, and some enrichment from the white side is no doubt occurring at the present moment. The thousands of English speaking troops in New Guinea can be trusted to speak Pidgin badly and so to enrich the concepts as well as the slang of the language.

On the native side it is more difficult to see how the enrichment will come. The language, however, already contains a certain number of terms for dealing with purely native concepts. There is, for example, the word *kandare,* for a classificatory mother's brother, *tumbuan* for a masked figure, *marsalai* for the New Guinea equivalent of the Rainbow Serpent, *poroman* for a matched companion, and a few others. Terms of this sort are already a help to the anthropologist and administrator who must discuss native life in Pidgin. For any given tribe, it is easy to add new words and special concepts from the local language to the Pidgin vocabulary, and this, perhaps, is a valuable contribution which the anthropologist makes. More could be done by a serious study of the features common to large numbers of New Guinea tribal systems, so that these common features at least could be labelled in Pidgin and so brought to the attention of the white man.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1944

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1944, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS:

(1) Eligibility. Authors and coauthors shall be members in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to **October 1, 1944**, to the Executive Secretary of The New York Academy of Sciences, at The American Museum of Natural History, Central Park West at 79th Street, New York, N. Y.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the

prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1944 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 6 MARCH, 1944 No. 5

SECTION OF GEOLOGY AND MINERALOGY[1]

FEBRUARY 7, 1944

DOCTOR PHILIP B. KING,[2] U. S. Geological Survey, Washington, D. C.: *Recent Studies of the Structure of the Folded Appalachians in Tennessee.* (This lecture was illustrated by lantern slides.)

The three northeasternmost Tennessee counties (Johnson, Carter, and Unicoi) lie in the Appalachian Mountain province. The rocks of the three counties, like those of the rest of the province, are strongly deformed, and the area is outstanding for its display of large-scale overthrusting. The thrusts involve pre-Cambrian, Cambrian, and Ordovician rocks, but are especially prominent in the belt of outcrop of the Lower Cambrian rocks.

This paper is an outgrowth of recent investigations of manganese deposits of the area by the Geological Survey and Tennessee Division of Geology. The results of the work, in so far as they relate to the manganese deposits, are soon to be published as a bulletin of the latter organization. A part of the investigation consisted of mapping extensive areas of outcrop of the Lower Cambrian and adjacent rocks, one product of which was a reasonably comprehensive new picture of the structural features of the region.

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[1] The Section of Biology met February 14, 1944. Doctor M. B Howorth gave an address entitled, "Some Biological Aspects of Mountaineering," illustrated by motion pictures in color and demonstration of climbing equipment. An abstract will be published in the April number of the Transactions.

[2] Published by permission of the Director of the Geological Survey, U. S. Department of Interior.

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This newer picture, in general, resembles that presented by Arthur Keith in the Roan Mountain, Greeneville, and Cranberry folios of the Geological Survey, but there are amplifications and modifications of the details. Movement along the Iron Mountain fault, which dips northwest, was interpreted in the folios as having been to the southeastward. However, distribution of rock facies, the nature of the minor structural features, and other evidence indicates that the movement was actually northwestward. This conclusion implies a much greater general movement of thrust sheets to the northwest than was originally believed. The rocks to the northwest and above the Iron Mountain fault are now interpreted as belonging to the Shady Valley thrust sheet, and the rocks to the southeast and below it as belonging to the Mountain City window. As the rocks of the thrust sheet originated southeast of those of the window, the displacement by overthrusting was more than 15 miles. Resting on the Shady Valley thrust sheet, as first recognized by Keith, is the still higher Bald Mountain thrust sheet.

As the thrust sheets have pre-Cambrian rocks at their bases, and extend southeastward into the pre-Cambrian, they probably originated as shears in the basement rocks. In this respect they differ from those farther northwest in the Appalachians, most of which have at their bases such incompetent formations as the Nolichucky or Rome. The latter probably originated as bedding-plane slips within the sedimentary succession.

The pattern of the overthrust faults is complicated by many tear faults, some of great length and displacement, which had not been recognized before the present investigation.

SECTION OF PSYCHOLOGY

February 21, 1944

Mr. Lawrence K. Frank, New York, N. Y.: *The Emergence of Personality.*

This is a brief statement of an approach to the study of personality dynamically, as a process, not as a thing or collection of discrete traits or motivation or various "mechanisms." It is the product of many years of effort to study the personality as a process, which emerges from the impact of culture upon the growing, developing maturing human organism, viewing culture as the traditional beliefs and practices which are incorporated into that developing organism, thereby giving its orientation to the world.

The organism may be considered as existing in a multidimensional environment—the biosphere—as described by Doctor Angyal, wherein we may distinguish (a) the geographical space of the natural world, (b) the internal environment of the organism, (c) the culturally patterned environment, (d) the socially organized environment and (e) the "private world" of the individual personality or the "life space" as Kurt Lewin has called it.

The newborn infant grows in size and weight, changes its shape and develops various new capacities, but, at birth, thanks to his mammalian ancestry, he has almost fully developed physiological functions, including the capacity for emotional reactions. These physiological functions are very labile and not well co-ordinated, however, so that the infant, during the first year or two develops his homeostatic capacities through a progressive integration of functions. During gestation, the fetus has been protected, kept warm, sheltered and continuously fed, while it has rehearsed or practiced many of the activities and the processes which it must perform after birth. While birth may be regarded as an incident, it also is a period of transition, imposing a necessity for drastic readjustments both internally and externally, as the newborn begins to breathe air and to "take over" and change its circulation and otherwise depend upon its own physiological processes.

As a young mammal, the infant has physiological, functioning capacities and certain organic needs, such as sucking, cuddling, mothering and tactual contacts. Depending upon the cultural patterns and sanctions, he will either be provided with the satisfaction of these organic needs or will be deprived of them and will adjust or react accordingly. He will also have to submit to the feeding practices of his parents, which may provide breast feeding or other forms of feeding, at intervals determined by them, so that he must accommodate his physiological functions (utilization of blood sugar, etc.) to these feeding practices. Likewise, he will undergo some form of training in control of eliminations. He is expected to learn to inhibit the automatic release of the sphincters, to recognize accumulating pressures as cues to call for help, to seek the appointed vessel or place for urination and defecation. He will also undergo a certain kind and amount of training in the management of his naive, emotional reactions to these experiences, accordingly as his parents either permit or forbid their expressions.

In these early homely practices of child rearing, we witness the processes by which culture, as transmitted by the parent, is incorporated into the growing organism so that his internal environment becomes "structuralized" or adjusted more or less as required by the cultural patterns. This involves a surrender of some of the child's physiological autonomy, derived from his mammalian ancestry, and the acceptance of external controls which thereafter regulate the frequency and efficiency of those functional processes. It is important to remember that this patterning of the internal environment is a modification of the well-developed physiological processes in the infant so that these regulations and modifications will tend to persist throughout the life of the organism. Moreover, this patterning involves deprivations, frustrations and coercions which may provoke emotional reactions, especially if the infant is not comforted, reassured and so helped to accept these interferences.

It may be pointed out that emotional reactions are total organic reactions which usually impose a heavy physiological cost upon the reacting organism by using up its energies. It is therefore impossible for an organism to continue "emoting" for any length of time since that would bring early exhaustion, which, however, sometimes happens to some children. When faced with more or less continual provocation to emotional reactions, the organism may become anaesthetic and cease to react to certain biological provocations (especially if it can develop some way of meeting such situations), or it may develop a persistent affective reaction which we may call anxiety or guilt or resentful hostility. These persistent reactions are subacute emotional reactions which are carried by the organism while engaged in other activities. They may be localized in one or more organ systems or functional processes, such as the gastrointestinal tract, digestion or eliminations, breathing, heart and circulation, and so on (the so-called psychosomatic disorders or dysfunctions). Thereby, the physiological functions we call emotions are modified into subacute or disturbed processes and functions.

By the foregoing practices of child rearing, the biological hunger of the child is transformed into appetite for preferred foods eaten at intervals established by the culture. Eating may thereby become either a weapon or a defense, a source of gratification or of unhappiness, or an occasion for other interpersonal relations. Likewise, eliminations are patterned into regular controlled voidings and become occasions for modesty, cleanliness or shame. Thereby, they become ways of pleasing or of defying parents and become loaded, so to speak, with the affective reactions of the parent-child relationship.

It is appropriate to suggest that this surrender of physiological autonomy and loss of internal self-regulation brings certain gains and advantages. It frees the individual from coercion of his own organic impulses and physiological demands, so that he can be free to engage in other activities, instead of seeking food continually and eliminating haphazardly or being emotionally provoked as are most other mammalian organisms. These are basic to group living or social order but impose certain burdens upon the individual.

As the child begins to move about and explore the environing world, he undergoes the further cultural lessons of learning to observe the inviolabilities of things, places and persons which we call private property, the integrity of the person and the sacredness of specific places. A child learns these lessons through the continual instruction by the parent, who says "don't touch," "don't handle," "don't strike," "don't manipulate," thereby blocking the child's naive and impulsive approach to things and persons, punishing disobedience of such commands and rewarding compliance. Under this tuition, the child learns, more or less effectively, to transform these parental prohibitions into self-administered repressions, so that he can tolerate exposure to biologically provocative stimuli-situations without reacting overtly to them (this learning of inviolability has been experimentally demonstrated in perch by Triplett some forty years ago, and confirmed more recently by Gray in Cambridge, England). Concurrently, the child must learn that, for every person and every situation, there is a prescribed pattern of conduct, and so he learns, under parental teaching, to utilize the prescribed form of manners, etiquette and symbolic, ritual conduct appropriate in each instance, including the early stages of the masculine and the feminine roles. He likewise learns his status and what to do to and to expect from other individuals, both his kin and others, and he learns to respond to language and to use language, as a medium of communication, manipulation and defense.

In these lessons, we see how the parent, as cultural agent, defines every situation and every person for the child, in terms of what he must *not* do, what he must do, what he may do, and what he should do. In this definition, parents exercise what is called authority, invoking the sanctions they deem appropriate to make those lessons effective, including the administration of punishment and the giving of rewards. These definitions of situations and the prescription of the appropriate conduct "structuralizes" the external environment of things and people and relationships so that the child learns to live in a culturally defined world which becomes invested not only with the meanings given by cultural traditions, but also with the authority of the parents who give that teaching. Thereby, the naive impulsive behavior of the young organism becomes transformed into the orderly, patterned conduct of the cultural traditions and the child learns, therefore, to live in this culturally defined world and to react with all the persistent affective reactions which he has developed while undergoing these deprivations, frustrations, coercions, punishments, and whatever reassuring or compensating love and protection he may receive.

While undergoing these complicated and perplexing lessons, the child also is given the basic ideas, beliefs, and assumptions of his culture. The child asks "why" and the parent says "because," and then proceeds to tell the child his or her understanding of the religion, the philosophy, the law, the folklore and customs of the group, plus any scientific or technological knowledge of events. It is important to recognize, however, that, while there may be a more or less "official" or authoritative formulation of these basic ideas and beliefs (expressed in literate cultures in authoritative books), the parent, in accordance with his or her sex, rank, class and social-economic status, regional origin, sectarian preference and family bias or peculiarities, will necessarily present the child with a variation of the official culture. Moreover, the parent will be biased in this teaching with peculiar feelings of tenderness or rejection for each individual child and with greater or less concern over certain specific phases of this early instruction, such as eating, or eliminations, or lying, or the untouchableness of the child's own genitals. Therefore, each individual parent will further warp and distort the official culture in transmitting it to a specific child. The individual child, however, will learn from these lessons what these experiences mean to him and how he feels toward the parent, toward each particular lesson, and the sanctions including the rewards or punishments invoked. It seems appropriate, therefore, to say that each child develops a peculiar, idiomatic, personalized version of the cultural traditions and, of necessity, learns to interpret and to utilize the various symbols and rituals and patterns of conduct, and all the reasons given therefore, in his own warped, distorted, incompletely understood, personal manner. Thus, we may say that he builds up a "private world" of his own, in which every person, object and situation will be seen, will be understood, and will be reacted to as they appear in his "private world." He will necessarily have a selective awareness, which focuses his interests and attention on certain people and situations and events of particular significance for him and makes him ignore or be insensitive to all other aspects of situations and people.

Personality, therefore, may be regarded as the dynamic process of creating and maintaining and defending this "private world" in which each individual lives, wherein each one is reacting affectively to every situation in accordance with the meaning, the significance and the provocations of feeling which his life experience has given them.

It may be helpful to remember, in this connection, that while there is an official language with its grammar and rhetoric and spelling, yet each individual learns to use that official language in his own highly, idiomatic way, using sentences, phrases and intonations which are peculiarly his own and yet are usually sufficiently like the official language and the usage of others to permit of communication. Likewise, each person utilizes the common patterns of culture, but in his own way and with his own personal meaning.

It should be added that, as the child grows older, he learns to utilize the socially sanctioned rituals, practices and symbols which have been established for regulating and controlling all interpersonal relationships, such as contract, barter, sale, employment, rental and courtship, marriage and divorce, and voting, litigation and all the other social practices through which the individual carries on his activities within the public world of social life while living in his "private world," that "life space" which has been organized and "structuralized" and usually distorted by his life experience.

Time does not permit any extensive discussion of the further experiences in adolescence, which, in our culture, frequently involve profound disturbances in the life of the individual, necessitating unlearning much of what has been painfully acquired in early childhood and accepting the new beliefs and patterns of conduct demanded of the individual for adult living. In our culture, this involves emancipation from dependence upon parental care and control in order to become more independent and self-directing, in order to become an adult who can live with ones contemporaries, accepting the responsibilities and opportunities of adult life. It also involves clarification and acceptance (or sometimes the rejection) of the masculine and feminine roles, the development of relations with members of the other sex which may arouse acute anxieties because of the shame and guilt associated with genitals and sex functions in early childhood. It also necessitates a sometimes drastic reorganization of the child's picture of the world and a reconsideration of all the phantasies and illusions about his parents, religion, government, business, social life generally, with a concomitant alteration of the image of the self. During adolescence, the individual personality may more or less crystallize his or her "private world," thereby fixating all the misconceptions and inadequacies of his childhood learning and also fixating the persistent affective reactions of anxiety, guilt, resentful hostility, which, if not released or expressed in more direct overt conduct, will find resolution in the various socially sanctioned occupations, careers, ambitions and interpersonal relationships which are offered by social life to the adult, or in physiological dysfunctions called psychosomatic disorders.

The foregoing is offered, not as a pronouncement or an explanation, but as a theoretical formulation to guide further study and reflective thinking upon the problem of personality development and expression. It has the advantage of viewing the organism in its multidimensional environment and observing how both the internal environment and the external environment become structuralized or patterned and otherwise modified, as the growing, developing organism undergoes the process of being culturalized and socialized. It has the further advantage of avoiding the frequent practice of reifying organic functions into the entities and endowing those entities with casual powers. It avoids the usual practice of imputing or attributing discrete motives to individuals as explanations for their conduct. It attempts to avoid the usual practice of fractionating the organism into several seemingly distinct but interacting parts, such as the old mind, body, soul dichotomy. It, likewise, abstains from hypostasizing various organic functions and capacities and from invoking the variety of "mental mechanisms" now relied upon in many current theoretical approaches. It specifically asserts that past experience exists not "out there," but is built into the developing organism, where it continues to exist in the modified structures, functions, behavior and feelings of the organism. Recent experiments with hypnosis have shown that this past experience is in the individual and can be revived. It suggests that this past experience, or forgotten childhood, and the persistent mammalian functions, which have only been partly brought within the coercive control of cultural patterns, may be regarded as what we now call the "unconscious." This would suggest that what we call the conscious, or consciousness, is the process of talking to ourselves—that continual inner monologue through which we strive to formulate all our experiences and our reactions in verbal terms, so that any of our experience which has not yet been verbalized might be regarded as still "unconscious."

Finally, it asserts that we exist in the present—that there is only the present, but that we live by anticipation, that is, we react to this present in terms of the meaning of events and what they either promise or threaten. This continual appraisal of the present in terms of the future, and our attempts to attain or to evade the consequences of the present, is one of the major activities of the personality process, a fundamental characteristic of our "private world." In this appraisal of the present, we are, however, directed, if not controlled, by our past experience which provides the criteria for our evaluation of situations and events and, therefore, serves to give the present its most compelling dimensions. In this way, we may view the personality in the "private world" it creates as having not only a spatial perspective but a time perspective, which is governed by the individual's life experience and by the larger cultural patterns, that we call the values of our traditions.

The personality emerges as a dynamic process which the human, mammalian organism, with his relatively large brain, develops under the impact of cultural traditions as transmitted to him during the period of childhood and adolescence, so that he learns to create to maintain a "private world" in which he really lives, while participating in the common public world with other individuals.

* * *

For any reader who may be interested in the fuller development of this approach to personality, the following papers by this writer are cited:

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SECTION OF ANTHROPOLOGY

FEBRUARY 28, 1944

PROFESSOR RAYMOND KENNEDY, Yale University, New Haven, Conn.: *Applied Anthropology in the Dutch East Indies.*

The science of anthropology has been traditionally concerned with so-called primitive peoples and cultures. Although some anthropologists have extended this basic interest to include study of modern American and European societies or advanced Asiatic cultures and historical civilizations, most of them have concentrated their research, teaching, and publication upon the non-literate, non-"Western" societies of the past and present. Since the great majority of these peoples live in the colonial areas of the world, whenever practical application of anthropology is being considered, the best immediate possibility of such application is usually seen in the sphere of colonial administration.

The Netherlands East Indies give us probably the best test case of anthropology applied to the administration of a colonial possession. The East Indian civil service has been the only official body in the world that has made the study of ethnology, native languages, and native law compulsory for all its staff. Candidates for the service were selected by open examination from among high-school graduates in the Netherlands and the Indies, and successful applicants were assigned to either Leiden or Utrecht University for a five-year course. This course included intensive instruction in the ethnology of Indonesia, in native and Islamic law, and in at least two of the languages of the islands: Malay, the *lingua franca* being required of all, and the second language open to choice.

When the candidate for the service completed this course, he was thoroughly conversant with Indonesian culture, and, upon landing in the Indies to take up his duties, he was prepared—anthropologically and linguistically—to deal with the problems of native administration on the Indonesian level and from the Indonesian point of view. Before becoming a full-fledged "contrôleur" or district officer, however, he had still to undergo a period of local apprenticeship as assistant to an experienced official. When he passed this "aspirant" stage, as it was called, he joined the ranks of the service on the lowest level of "contrôleur," with a small district to supervise. From then on, his career up through the grades to assistant resident, resident, and perhaps governor of a province depended upon his ability and performance.

Thus, every official in the East Indian civil service was a trained anthropologist. This statement can be made unreservedly, because no one who had not passed the standard course at either Leiden or Utrecht was allowed to take the examinations for the service.

In these days, when, in our country, scholarly specialization would seem almost to have become a hindrance rather than a help in obtaining government positions, and when, in official circles, the unlearned but "practical" man is glorified to the denigration of the professor or the holder of a doctor's degree, it is especially interesting and pertinent to examine the results of the Dutch policy of appointing only university graduates with highly specialized training to administrative posts in the East Indian service.

What were the results? The general answer is that among colonial administrators of all nationalities, the judgment was virtually unanimous that the Dutch civil service in the islands was the best and most efficient in the world. The Dutch knew more about the Indies than any other colonial power knew about its overseas possessions, and their skillful administration of the islands was the envy of other nations. The colonial policy of the Netherlands might be criticized sharply on several important points, but the actual administration was a model of smooth efficiency.

If we factor out other considerations,—the long and almost uninterrupted occupancy, the relatively untroubled home-country politics, and the traditionally benevolent attitude of other nations toward the Netherlands—we are left with what I consider to be the most important element of all in accounting for Dutch success in the administration of the Indies: a deep and sympathetic understanding of native attitudes and native ways on the part of all those charged with governing the islands and their extremely varied peoples. I make this statement because I believe that, in the last analysis, *the* colonial problem is the native problem. It is a matter of handling human beings, and human being with customs and views widely variant from those of the mother country. And that is where the anthropological training of the Dutch civil service staff in the Indies paid rich dividends. Smooth "native-level" dealing with the Indonesians was, I believe, the master key to Dutch success in the administration of Indonesia.

The results of this application of anthropological knowledge and the anthropological point of view can be seen in many specific instances. In the first place, the Dutch displayed a remarkable understanding and tolerance of native customary law. They applied themselves diligently to mastering the intricacies of the *adat,* or traditional law, of each of the parts of Indonesia. Finding that there were great variations in different tribes and sections, they drew up a special set of laws for every region, in which full consideration was given to the local standards; and changes were made only where these were deemed intolerable, as, for instance, in the case of trial by ordeal or the death penalty for minor offenses. Thus, each native *adat*-law territory had its own rules, and, furthermore, special law codes and courts were established for Europeans, Chinese, and other foreigners resident in the islands. The work of legal research went on continuously, for, as various parts of the Indies became progressively more "Westernized," the laws were correspondingly altered to suit new conditions. The Dutch literature on Indonesian customary law is of impressive bulk and quality, to the extent that Netherlands scholarship stands unparalleled in the general field of primitive law.

The tolerance and understanding that anthropology breeds can be seen also in the Dutch treatment of native religion. Here, again, we find the same painstaking research, in the form of a mass of literature, including source material and interpretation, and the same application of knowledge, as in the case of native law. The colonial administration took full cognizance of local religious beliefs and practices in all its dealings with Indonesians, and seldom made a false step on this risky ground. This applied not only to the bewildering variety of pagan cults in the islands, but to Mohammedanism and the vestiges of Hinduism as well. All Christian missionaries in the Indies were carefully supervised, and their licenses were revocable whenever their proselytizing zeal appeared to overstep proper bounds, or whenever the natives of a district desired their removal. Certain parts of the islands—strongly Mohammedan Atjeh in Sumatra and Bantam in Java, and Hinduist Bali—were closed to missionaries. Missionaries were tolerated, just as native religions were; but the latter, with priority rights, so to speak, received primary consideration.

In line with the Dutch tolerance of native religion, the East Indian government never tried to tamper with the private morality of the Indonesians. They could dress as they pleased, or as little as they pleased, practice polygamy and trial marriage, and perform their rituals, however "indecent" these might appear to foreigners, without interference. The meddlesome puritanism that has afflicted some other colonial governments—and our own Indian service in years past—had no place in the Indies. The Dutch insisted that these matters were the natives' own private business.

A very important economic consequence of the Dutch knowledge of native culture appeared in the laws regulating property in land. The Indonesians, like most so-called primitive peoples, hold land communally. Private property in the soil is unknown to the *adat,* and each village community owns its land as a group. The Dutch have supported this traditional system of landholding, and they have refused to allow any alienation of native land to outsiders. The foreign enterprises in the islands operated plantations and mines on long-term leaseholds, the proceeds of which went partly to the government and partly to the native proprietors. The result of this firm insistence on keeping land ownership in the hands of the Indonesians is that the native economy has remained basically sound, for the people, predominantly agricultural, have never lost possession of the soil from which they draw their sustenance. Our American policy with respect to American Indian land presents a sad contrast, as does the land situation in the Philippines, where the traditional communalistic system of land holding was violated long ago by the Spanish, and where great numbers of impoverished tenants and sharecroppers form a permanent slum population.

It would be possible to go on and give many other instances of how knowledge of native attitudes and customs has been applied in the administration of the Indies: how, for instance, the governmental system itself was developed largely out of traditional native patterns in the form of democratic village councils, indirect rule by way of native chiefs and princes, and regional councils established on tribal lines; or how the weaknesses of the Indonesians for pawning, opium smoking to a small degree, and gambling were tolerantly handled with a view to systematic improvement rather than drastic suppression. I am inclined to think also that the extremely liberal racial code of the Indies was maintained largely because of the enlightenment of anthropologically trained officials.

Is there then no dark side to this picture? Does anthropological training engender no defects, no blind spots, in colonial administrators? Although I hesitate, as an anthropologist, to admit it, I do believe that one weakness revealed itself among the anthropologically trained Dutch officials in the Indies. The results of their administration were: intact and flourishing native cultures, a just and comprehensible legal system, a basically sound native agricultural economy—and, unfortunately, an Indonesia so far behind the times that it was helpless, unless protected by some strong, modern, outside power. In their highly commendable zeal to help the Indonesians preserve their traditional culture, the Dutch anthropologist-administrators sheltered them too much from the outside world. They cherished the native cultures to such an extent that they were blind to the need for adjustive change in these cultures to suit changing world conditions. This, to my mind, is the one great danger in having anthropologists as administrators. They may do more harm than good to native societies, in the long run, by insulating them from outside trends and forces, because these pressures are almost certain to push in upon the people sooner or later, and they will be caught unprepared, in all their pristine primitive serenity.

Interestingly enough, the anthropologist-administrators were not alone in desiring that the Indonesians remain "primitive." The Dutch financial interests in the Indies also favored this policy, for they saw realistically that by keeping the Indonesians "under glass," as it were, they could insure themselves against the two things they feared most: nationalistic agitation for independence, or, at least, a large native share in the government; and native demands for economic betterment, higher wages, and increased expenditure on public welfare. Encouraging "primitivism" in culture, and maintaining the tribal heterogeneity of the native peoples, so that they would not develop "pan-Indonesian" nationalism were excellent means of circumventing this double threat. Thus, the sympathetic attitude of the anthropologist toward native cultures and the self-seeking motivation of the business interests worked hand in hand.

An additional purpose served by the "anti-acculturation" policy of the Dutch was economy in administration, particularly in regard to education. The government never spent more than ten per cent of its budget on education, in contrast to the twenty-five per cent allowed to schools in the Philippines. Moreover, most of the education given in the Indies, again under the guise of fostering native culture, was confined to technical subjects and Indonesian arts and crafts. A very small number of native pupils ever attended the higher schools, and almost none rose to college level. The Dutch were preparing them, it would seem, not for independent, self-reliant citizenship in a dynamically changing world, but for a kind of permanent wardship under a benevolent, paternalistic colonial administration. Only a minute proportion of Indonesians ever acquired knowledge of what was going on in the world outside by study of such subjects as international politics and economics.

And so I believe that applied anthropology was carried almost too far in the Indies, and begot what I have termed an "anti-acculturation" attitude among the administrators who held the fate of the Indonesians in their hands. Therefore, if I were to propose that all colonial administrators of the future should be trained in anthropology, I would also insist, with the Indonesian case in mind, that they be required to learn the facts about acculturation. It happens that I am a sociologist as well as an anthropologist, and, while I am sure that I suffer in some respects from this split personality structure, my sociological training has made me see clearly the danger of trying to deal with colonial peoples on a purely anthropological or ethnological basis; that is, paying attention entirely to their traditional culture and not viewing them in the perspective of world events and trends. This emphasizing of acculturation is particularly important now and will become increasingly so in the future, as the world shrinks more and more rapidly. There are few "far corners" left where archaic cultures can live on in undisturbed peace. And, in this narrowing world, there is a pressing need for more sophistication, more consciousness of rough reality, on the part of the native peoples of the Indies and other colonial areas. Otherwise, ignorant and defenseless, they will continue to be what they are now, mere human booty of imperialistic war.

Discussion of Dr. Kennedy's Paper

By Claire Holt

East Indies Institute of America

It seems to me that Dr. Kennedy's stimulating talk raised a few points which could be carried further in discussion.

He has outlined how thorough anthropological training of Netherlands East Indies administrators enabled them to establish and maintain harmonious relations with the local population. In many cases, their understanding sympathy for indigenous cultural institutions and attitudes produced the sort of familiarity which inevitably creates an emotional tie, in some cases a real love, be it for the refined Javanese, the rowdy Bataks or the dignified Toradja.

During my stay and travels in the islands of the East Indies, I was able to observe the eager, almost jealous absorption with which a young "contrôleur" would suck in the peculiarities and details of life surrounding him, fitting them lovingly into the mosaic of patterns which gradually filled in what he knew to be the general framework of native society. It was the joy and pride of many an older and higher official to be able to discuss the finer intricacies of native psychology and behavior.

Dr. Kennedy says that this attitude, based on anthropological training, was carried too far. Insufficient or no attention was given to the necessity of viewing native culture in the perspective of world trends and events. This produced what he termed an anti-acculturation attitude among the administrators "who held the fate of Indonesians in their hands."

Did they hold the fate of Indonesians in their hands? No, and yes. One is tempted here, first to indulge in an old platitude: Is not every science in our society subject to misapplication?

Research and invention in the fields of chemistry and physics, which could be of greatest benefit to humanity, can be and are being applied for the destruction of human life and of laboriously built-up means of sustenance.

Similarly, anthropology can be used by governments as one of the most useful sciences for furthering an enlightened policy of progress, as it can also be used, quite consciously, or less consciously, to retard it.

Wouldn't it depend a great deal on the point of view given to the anthropologically trained administrator at the source, while he was being trained?

Doesn't it make a difference whether an anthropologist is sent out into the field charged with the task to study a native community (full stop!); or whether he be charged to study the native community with a view to introducing an innovation in its midst—a new school, for instance?

This puts the burden of responsibility, not on the anthropologist himself, but on the makers of policy. On the other hand, however, the anthropologist-administrator, who goes out into the field with an order ending with the "full stop," can and should have enough perspective and knowledge of trends in a changing world to keep these in mind while studying conditions. Next to facts, he will then be able to crystallize recommendations—which would, or would not, be put into effect.

In contemplating the scene of applied anthropology in the Netherlands East Indies, one cannot deny that, in many instances, these two processes were in effect: namely, instruction from higher instances to the field worker; and, vice versa, recommendations from the field to the authorities.

Transformation of Balinese school-houses, from drab bungalows into gay structures with pyramidal roofs in Balinese style, was one of the results of the second process.

Similarly, changes were made in the method of art instruction. Formerly, it was based on school methods of the motherland. Children sat copying European designs or drawing from plaster casts and objects—pitchers, bowls, bottles and whatnot—which bored them to tears. In recent years, they were permitted to turn toward the art of their ancestors and play with motifs which greeted them on their own temple walls.

The policy in the question of missions in Bali, where activities were restricted as they threatened to disrupt community life, was the result of interplay between recommendations issuing from the field to the authorities, and vice versa.

But, obviously, such and many more similar instances, while good symptoms, are not yet satisfactory from the point of view of those who would like to see an all-pervading tendency to enable Indonesians to take the greatest share of responsibility in their own government.

One of the drawbacks in this direction, pointed out by Dr. Kennedy, is the insufficient amount of education.

And, to my mind, this raises another most vital question. At first glance, education seems to be a clear, unequivocal concept. Yet, upon closer examination, as you all know, it is far from simple. Advertisers of poisonous beverages or "coffin-nails," as cigarettes are sometimes referred to (to which I am myself an addict), also speak of "educating the public."

The problem of education in Indonesia, and for that matter in any non-Western society, as viewed from here, and also by any administration responsible there, is very complex.

I should like to come back for a moment to the Netherlands East Indies administrator—anthropologically trained to be sure—whom we accuse of excessive tenderness toward native ways. I think that it might be good to realize that, in some instances, living with the so-called native mentality is a wonderful escape and relief for many a Westerner.

It is not sheer sentimentality which makes us sigh for the unharassed pace, the more contemplative state of mind and the gift for indolence (which does not mean, as the legend goes, that the people are lazy).

It is clear that there are certain characteristics and attitudes among the Indonesians which make Westerners plainly envious. And those who can look deeper and touch the roots from which these attitudes spring, realize only too clearly that "education" may become a calamity instead of a blessing.

It is easy to produce efficient clerks, human adding machines, skilled mechanics, good foresters and tax collectors. They are doubtless needed and useful. Also doctors, lawyers, teachers and government officials can be trained in ever increasing measure, should the education budget be raised from 10 to even 40 per cent of the country's income.

In all this, however, specialized skill can still be insulated from the body which makes up society and its ethos. We often see this right here, even in our own country.

Granted that history has produced the circumstances in which what is termed "guidance" is inevitably needed for our bronze-skinned fellow-beings in the tropics, the greatest problem is the one loosely called education.

I remember a concrete instance. When an experimental step in a new direction was taken in Java, and one of the Government-sponsored schools was set up with a program including Javanese culture history, oriental religions and languages, objections were voiced from two opposing directions. There were some conservative voices saying that the introduction of subjects concentrating on indigenous culture would produce nationalists; but at the same time, some of the radical elements among the nationalists said this program was designed to retard progress and to withhold the benefits of pure Western education from the Javanese students.

It would of course be as wrong to follow a purely Western program of education, as it would be undesirable to concentrate exclusively on the assets of indigenous culture.

Wherein lies the synthesis? For only a synthesis of what is best in our education with what is beneficial and even admirable in Indonesian culture can produce a society with a face and dignity of its own, and capable of vital participation in the international world.

The indefatigable research of Netherlands East Indies anthropologists and the immense stores of facts and information they have accumulated can be utilized to the fullest extent in shaping educational policies; and, in the process, quite a few things may be discovered which could be profitably borrowed for our own educational system.

One of them is the deep-rooted feeling of communal bonds. Beginning with the activities in the village communities Dr. Kennedy mentioned, and carried through in every aspect of work, Indonesians love to do things "bersama-sama," meaning "together with each other." Doing things "bersama-sama," in contrast to our emphasis on concentrated individual effort, could make Indonesians ideally co-operative with anyone who really means co-operation.

I do not think for a moment that anthropological master minds will shape the eventual ideology of peoples, even though modern history has shown that ideology can be forced upon and fostered in a nation. History has shown the slow processes of eventual selection and adaptation which transformed civilizations. A complexity of forces, over a span of time, molds new forms.

However, the question put here is in essence: Is it the role of the anthropologist merely to collect and study facts? Or is it also to consciously help formulate desirable social changes and the means to attain them?

SECTION OF PHYSICS AND CHEMISTRY

February 11 and 12, 1944

Conference on *"Energy Relationships in Enzyme Reactions."*

The Section of Physics and Chemistry held a Conference on "Energy Relationships in Enzyme Reactions," as the first in the series for the Academic Year 1943–1944. Doctor Joseph S. Fruton, Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by Joseph S. Fruton.

"Energy Relationships of the Oxidative Enzymes," by Eric G. Ball, Harvard University Medical School.

"Energy Relationships in Glycolysis and Phosphorylation," by Otto Meyerhof, University of Pennsylvania, School of Medicine.

"The Function of Phosphate in Enzymatic Syntheses," by Herman M. Kalckar, Public Health Research Institute of the City of New York.

"Significance of Coupled Reactions for the Enzymatic Hydrolysis and Synthesis of Proteins," by Max Bergmann and Joseph S. Fruton, Rockefeller Institute for Medical Research.

"Enzyme Reactions of Sulfur Compounds and Their Possible Interrelationship," by Carl V. Smythe, Rohm and Haas Company, Philadelphia.

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ELECTED FEBRUARY 24, 1944

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TRANSACTIONS
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SER. II, VOL. 6 APRIL, 1944 No. 6

SECTION OF GEOLOGY AND MINERALOGY[*]

MARCH 6, 1944

DOCTOR GEORGE H. CHADWICK, University of Newark, Newark, N. J.: *The Geology of Mount Desert Island (Acadia National Park)*.[†] (Illustrated by lantern slides.)

Two-thirds of the way up the ragged coast of Maine from New Hampshire lies the second largest water-bound land-mass on the eastern shores of the United States—the oft-mispronounced island of *Mount Desert* (de-sert′, "isolated or solitary" mountains, derived from "l'Ile des Monts-deserts" of Champlain, the discoverer). Here alone in the East do the mountains meet the sea. And here, reveling in both silent mountain top and thunderous sea-cliff, lies Acadia National Park, our unique sea-coast Federal park.

Of roughly circular outline, but split nearly in two up the middle by Somes Sound, the Island comprises a hundred square miles. The low southwest lobe is entire, but, what should be the companion southeast lobe, east of the entrance to the Sound, is invaded by the sea and broken up into the five Cranberry Islands. Close along the northwest side is the large Bartlett's Island, separated by a steamboat channel. Better known are the five spruce-forested isles, the Porcupines, on the

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[*] The Section of Psychology met March 20, 1944 Dr. Ernst Kris delivered an address entitled, "Art and Regression" An abstract will be published in the May number of the Transactions

[†] The talk was illustrated by Kodachrome slides, as well as standard slides, loaned by the Acadia National Park Service The speaker, in addition to his geological subject, covered various features of the local history, scenery, and wild life of Mount Desert Island

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northeast, forming a horseshoe around the anchorage at Bar Harbor, from the hills of which they look like five of these bequilled animals swimming a race to the open Atlantic.

The central portion of Mount Desert, across which runs the mountain range (paralleling the slant of the Maine coast) for ten miles, is of warmly-tinted pink to red granite or quartz-syenite, surrounding which are the older, "weak" rocks that the granite intruded from beneath. But, as the granite mass is longer east-west, and the island itself is longer north-south (about 15 miles by 12), these weaker rocks are found chiefly in the north and the south portions. Indeed, the granite reaches the sea at points on the southeast and northwest shores.

The oldest rock exposed is on the west and northwest sides and on Bartlett's Island; thence north to Ellsworth and west to Deer Isle, beyond our limits. This is the crinkly, lustrous *Bartlett's Island schist* (wrongly renamed "Ellsworth" schist), generally of bright green chlorite and clear vitreous quartz, but the mica is sometimes brown or black biotite, rarely gray muscovite. The sharp plications of this rock trend hereabouts mostly north or northeastward. Microscope sections (Alling, Fluhr) reveal that the original lamellated "shale" was in part or wholly volcanic ash, and the quartz interlaminae (only 1 to 2 millimeters thick) were lit-par-lit injections from some acid magma. Sills of presumably the same ancient granite (which I am calling *Uncle Israel's*), very white in color and much crushed (cataclastic), as well as some interbedded true hydroclastic white or gray quartzites, occur on Bartlett's Island and on the main Island, besides "gneisses" that are the schist soaked by this granite. Moreover, on the north shore, from Uncle Israel's Point westward, are extensive masses of clear granite-gneiss ("white") in which, at one point, an unmashed remnant of the original rock is an amphibole-syenite from which the quartz may have been drained away into the lit-par-lit laminae in the schist.

Supposedly, before the igneous bathylithic invasions, this Bartlett's Island schist floored the remaining sedimentary rocks of the Island, though it has no exposures over the eastern half. These remaining strata (given four different names by Shaler, 1889) form a seemingly indivisible succession that the elder Crosby (1881) long ago called the *Frenchman's Bay series.* But they exhibit two contrasting and interfingering facies. Developed more largely on the north and northeast is the *Bar Harbor* detrital facies of thin-bedded (so-called "flaggy") or sometimes almost shaly-looking sub-metamorphosed quartzose silts, dully polychrome in cliffs but light bluish gray where wave-scrubbed. Developed more largely at the south is the *Cranberry Island* volcanic facies, of felsite (and porphyry) flows and ashbeds, with some tuffs and some amygdaloids. Yet interbedded with these are recurring layers of normal Bar Harbor detritals at widespread localities; and, correspondingly, the volcanics occur as interbeddings clear to the north shore—lavas at Star Point, thin ashbeds on the shore path at Bar Harbor (on Reef Point), and thick ones, conspicuously, out on Burnt Porcupine, besides many other places. The ancient land lay northward, the volcanic vents southward. Perhaps at Anemone Cave, on the southeast shore, we have the melange of one of the vents, much cut by later diorite and granite.

At Emery's Cove, on the north, this sedimentary series, lying at low easterly dips and barely metamorphic, comes almost into contact with the highly crinkled, highly metamorphosed Bartlett's Island schists dipping steeply westerly, away from it. Diametrically opposite, at Murphy's Hill, horizontal lava-flows lie directly on vertically uptilted eroded edges of the black schist. Thus, at both points of contact, we have record in unconformity of a profound intervening orogeny.

Low dips, gently rolling, but with small overthrustings, characterize these Frenchmen's Bay strata at north and at southwest; but southeastward, beginning even at the south edge of Bar Harbor village and reaching a climax on the Cranberry Islands, the dips soon approach the vertical and presently become overturned. This is *Acadian folding.* It is evident that such folding bespeaks a now vanished overburden of other deposits. Like witness is borne by the igneous intrusions now to be described.

First of these is the *Ireson felsite.* The pinkish-weathered Ireson *dikes* are mostly thin and not very numerous, but at least two of them may be seen at Reef Point, Bar Harbor. They represent continuing irruption of the same magma from which came the felsite flows and ashes, and, in part, they are therefore contemporary with those, but more largely so with the overlying strata now lost by erosion. Some of the latest of these Ireson dikes, as one northeast of Salisbury's Cove, are so coarsely crystallized as to prove a heavy over-burden and must be much later in age than any remaining stratified beds that they cut. These dikes all show displacement in the Acadian folding, nevertheless.

At Ireson's Hill, and its cliffs at The Ovens on the north shore, the felsite early lifted a part of the overlying sediments into a dome or lava-blister and congealed therein as a laccolith, a mile in diameter and two or three hundred feet high. The sedimentary floor is exposed at two points. The roof material appears to have been an ashbed of which bits remain. Fragments of what may have been other such Ireson laccoliths have been found included in the later granite at Otter Cliff and elsewhere.

Subsequent to the Acadian folding, came a great bathylithic invasion, not limited to Mount Desert. This formed a trap or "black granite," which I (1942) have called the *Prettymarsh diorite*. It seems to have occupied most of the ground that the granite later took over, but a good deal more besides. Such devouring of one magmatic mass (here the diorite) by another (here the granite) has been described by excellent investigators in other parts of the world and there seems to be no doubt of it on Mount Desert; but, in adjoining quadrangles, the diorite has been considered a "border phase" of the granite because of its partially marginal distribution. On Mount Desert, at least, the border of the granite is whiter, not dioritic, and the diorite is greatly invaded and inclosed by the granite, with marked chill on the latter.

This diorite takes many forms, perhaps cupolas, surely *chonoliths, laccoliths*, and *dikes*, its texture varying with the size of the body but so coarse in the larger masses (2 to 3 millimeter grain) as to prove thick cover, thicker than for the Ireson felsite. The Prettymarsh dikes are mostly massive ones, usually with welted surfaces, but the distinction from the (much later) Triassic dikes is not always easy to make. The diorite *laccoliths* conspicuous in the five Porcupines off Bar Harbor have been overlooked by all previous workers, or misinterpreted. In each of these islands, and also in Ironbound beyond them, the floor of Bar Harbor sediments (with some ash interbeds) is exposed at low tide on the south end or side, but dipping gently north so as to be submerged on the north. On the other hand, remnants of the roof strata are found here and there on these north sides. On Long Porcupine, the feeding dikes mushrooming out into the "blister" are splendidly exhibited, and here the columnar jointing, like that in our Palisades (though the diorite is a much older rock), can be seen even from Bar Harbor, two miles away. Sol's Cliff and Great Head are other known diorite laccoliths, on the main island. In these the floor sediments have been softened and soaked, or much disturbed and convoluted, as seen from the passing boat. Other diorite hills, taken for cupolas, may really be laccoliths.

Latest of the great intrusions is the *Cadillac granite,* mostly coarser than the diorite as if cooled beneath still thicker cover. Normally, it is a pink or rose-red hornblende-granite or quartz-syenite, but marginally and in its dikes and veins it is blanched, grows finer-grained, and may carry some biotite also. On Hadley's Hill, and in the quarry on the north end of Champlain Mountain, the granite has assimilated some of the diorite, but otherwise the contacts and contrasts with that rock are normally sharp, except where the diorite has been strongly and unmistakably *soaked.* The sediments are quite as strikingly soaked by the granite at some points.

The commercial granite at Hall's Quarry on the north end of Somes Sound seems to be in one of these contact or border phases—lighter-colored to "white," fine and even-grained, with biotite(but I have never failed to find hornblende also) and good "rift" and "grain." It seems to indicate that the roof of the bathylith came down as a "pendant" in this valley of the Sound, in which there are other roof-remnants in its most deeply ice-eroded passage, on Sergeant's Drive.

Around the main *bathylith* of Cadillac granite on Mount Desert, there are several other patches of this pink granite, differing in no particular, that have the form of *cupolas,* and are sometimes very steep-sided and high, some being circular but most of them more or less elongated. These are believed to be cupolas, indicating that the granite bathylith flares outward underground to bear these on its concealed skirts. Demonstrably, the main granite mass also goes precipitously down under the "weak rocks" roof, at east and west ends. The suggestion thus arising, that its exposed portion also was originally studded with cupolas and that these determined the location of the present peaks, is strengthened by two other observations. One is that in every intermontane valley that has been explored for them, remnants of roof-pendants have been found low in the side walls, but no trace of the dikes that Shaler (1889) "hypothecated" (his own expression) in these valleys as causal. The other is the much-debated sheet-jointing concentric with the forms of the present granite surfaces, except where these were undercut by the ice-lobes. In a sea cliff on Bartlett's Island, the granite was found in igneous contact beneath the contorted schist and the sheets paralleled the roof; an instance which other theories fail to explain. It is believed, therefore, that the sheeting gives a reliable guide to the original contact surface of the granite, and from this it can be proved that almost none of the granite has been removed by erosion.

The granitic lowland north of the mountains, moreover, which writers have tried to ascribe to peneplanation or to glacial erosion, was found to be so largely a mosaic of diorite in granite that only negligible erosion of the granite could have happened here.

The wonder of the transmontane valleys thus finds answer in normal atmospheric weathering-out of weak roof-pendants, preglacially.

The apophyses and external ramifications of the granite are extensive. Marvelous mosaics occur also along its lateral contacts. Frequently, fragments of the Prettymarsh diorite dikes stripped of their wall rocks are sunk down into its upper surface, deceptive until their abrupt terminations are found. Rarely, some of the wall rock, such as Cranberry Island porphyry, remains attached to these. A case of this occurs on Cadillac Mountain road on the west summit, over 1500 feet above sea.

Strangest of all the effects of the Cadillac magma is the *Seawall metafelsite* or "sugar granite" covering a large (central) portion of the low southwest lobe of the Island. Everywhere about its margins this pink "granite" (frequently as coarse as the Cadillac) grades imperceptibly into the normal blue Cranberry Island felsite porphyry (quartz-melaphyre). Never as dense and firm as the Cadillac, usually with plenty of tiny miarolitic(?) cavities and with a vague or "fuzzy" look to its crystal grain, often varying in color or texture in bands and streaks, it is instantly rejected by the quarrymen, but has fooled many of those who have mapped the rocks of the Island, including myself. That it is a product of pneumatolytic metasomatism of the felsite porphyry by emanations presumably from the Cadillac magma seems, today, indisputable. Dikes of the Cadillac cut it at Stanley's Lobster Pound. Rare minerals occur in it at Seawall Camp Ground and elsewhere.

Last of the igneous rocks are the Newark trap dikes of Triassic age cutting the granite and the older rocks alike, which attract the visitor's eye on the Mountain Road and on Schoodic Point, far east. Still later than these, are veins of clear quartz and green epidote.

Without fossils to guide, the ages of these rocks may seem mere guesswork. There is not time here to present the evidence, but the Bartlett's Island schist bears every mark of being a metamorphic equivalent of the Etcheminian Lower Cambrian of New Brunswick, its folding the *Taconian* orogeny. The Frenchman's Bay strata tie closely to similar rocks not far west and east that carry Clinton and Rochester early

Silurian fossils, with similar felsite flows and ashes. The Ireson felsite may bridge from this age into the Siluronian, followed by the Acadian orogeny. The Prettymarsh diorite has close affinity with the basalt flows interbedded with the Perry Upper Devonian strata at Eastport that contain fossil plants diagnostic of the true (Senecan) Chemung of New York and Pennsylvania (Montrose, equals Wellsburg). The best present guess at the Cadillac granite is Mississippian (approximately Windsor), but that is a long story of mis-correlations and orogenies, involving even a doubt whether the Cadillac bathylith may not be after all a *pseudobathylith*.

The glacial history cries aloud for someone to tackle it while road cuts and borrow pits are fresh, after intensive road-building. The glacial troughs (dorrs) of the lakes and Somes Sound are "U-valleys," but their rims are a long way below. The mountain summits and their bottoms are only where the side slopes of the preglacial V-valleys would seem to meet, except in the over-deepened rock-basins. Glacial erosion need not be invoked for the rounded summits (cupolas), the unsymmetrical Porcupines (originally unsymmetrical laccoliths in dipping strata), the precipices of Champlain and Beehive (steep-walled bathylith with vertical sheet-joints) and many other such cliffs. Rhythmic oscillations of the waning ice rather than separate glaciations will account for any genuine cases of till over clay or sand, and, of these re-advances, the Jordan's Pond and Great Pond deltas seem to mark a major one.

Of marine bars and beaches, that build today only on the landward side of the islands, splendid ones exist all unsung northwest of Hull's Cove and on the southeast slope of Ireson's Hill.

Much of the glacial history of Maine and the postglacial land-sea oscillation can best be worked out on Mount Desert Island.

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SECTION OF BIOLOGY

March 13, 1944

Doctor Robert Gaunt, Washington Square College of Arts and Science, New York University, New York, N. Y.: *Endocrine Factors in Water Diuresis and Water Intoxication.*[*] (This lecture was illustrated with lantern slides.)

It is recognized that the glands of internal secretion are important in the regulation of water metabolism and distribution, but the extent and mechanism of the interrelationships involved have not received exhaustive study. The antidiuretic role of the neurohypophyseal hormone is apparently well understood, but that of the other hormones is more obscure.

Certain aspects of the actions of the adrenal cortex, the thyroid, and the anterior pituitary in the regulation of body water will be considered here.

There are two common means by which the gross aspects of such problems are studied: (1) by measuring the daily voluntary water intake and urine output, i.e., the daily water exchange; (2) by observing the fate of measured doses of water given by stomach tube to fasted animals. When water is thus given in small doses, it will be referred to as a water diuresis experiment; when given in large doses, as a water intoxication experiment. Largely for lack of time, only water diuresis and intoxication experiments will be considered here.

These observations were made at different times and for different specific purposes, and consequently there are some variations in experimental details.

Water intoxication, a syndrome named and described by Rowntree[1] and his collaborators in the 1920's, can be produced by forcing water more rapidly than it can be excreted. Details will follow, but, in general, excess water leads to obvious distress, climaxed by convulsive seizures of remarkable violence. The syndrome is not entirely one of theoretical interest. No normal person, under normal circumstances, would likely consume enough water to induce intoxication. The consumption of lesser amounts of water has been reported to cause symptoms, either mild or severe, when renal function is impaired, when there is shock-like hypotension, when pituitrin is used as in diabetes insipidus, or in any condition in which there is depletion of the NaCl stores of the body. To that list can be added, almost certainly, the state of adrenal and of anterior pituitary insufficiency. Probably, the most common instance is the condition of heat cramps, apparently due to the loss of NaCl by sweating. Thirst is intense, and, if satisfied by water without replacement of the salt, leads to the well-known toxic symptoms, which seem to be those of water intoxication.

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EXPERIMENTAL TECHNIQUES.—In most of our diuresis experiments water was given in two doses at hour intervals by stomach tube. The doses were calculated either on the basis of body surface or body weight, but in either case approximated 5 per cent of the body weight. The body surface dose for a 200 g. rat was 9 cc.; the body weight dose, 10 cc.—a difference that is immaterial for the purposes of this discussion. Water intoxication was produced in adrenalectomized rats by 5 doses at hour intervals; and, in normal rats, by 13 doses given half-hourly.

RESPONSE OF ADRENALECTOMIZED RATS TO WATER

Rowntree and Snell[2] first noticed a lack of normal water diuresis, following the administration of water, in Addison's disease. The same phenomenon was noted and studied experimentally by Rigler[3] in mice, by Swingle *et al*[4] in dogs, and in our laboratory in rats.[5] In all cases, it was noted that adrenalectomized animals failed to eliminate administered fluid properly, and that a high susceptibility to water intoxication was present.

We have recently investigated the problem in some detail, with the following results:

DIURESIS.—Rats adrenalectomized for only one day—and hence having practically no signs of adrenal insufficiency—show a reduction of more than 30 per cent in the rate at which small (two) doses of water are excreted. When 5 doses are given, the resulting diuresis is over 60 per cent below normal, and definite intoxication symptoms appear.

If a week elapses after adrenalectomy, even though the animals are well-maintained on salt or cortical hormones, the diuretic response to water is lost almost entirely, and intoxication is still more easily induced.

BODY TEMPERATURE.—In both normal and adrenalectomized animals the progress and extent of a state of water intoxication can be followed reliably by the simple device of measuring rectal temperature. Temperature gradually falls 15° to 20° F. before death. In the subsequent experiments, water intoxication was arbitrarily said to be present when body temperature had dropped to approximately 90° F.

CAUSE OF DEFICIENCY IN WATER DIURESIS AFTER ADRENALECTOMY.—Stated in general terms, there seem to be two reasons for the inadequate elimination of water after adrenalectomy: (1) a sluggish emptying of the stomach and delayed intestinal absorption; and (2) an inability to excrete normally water that is absorbed.

In animals receiving only 2 doses of water and operated for only one day, the delayed absorption could account for practically all of the lack of diuresis. When longer intervals elapse after adrenalectomy, or when high doses of water are given, the absorbed water accumulates in the bodies of adrenalectomized animals—presumably due to renal dysfunction.

When fluid (0.45% NaCl) is introduced intraperitoneally, absorption is at a normal rate. Diuresis is almost normal in one-day operated animals, but is decidedly diminished in animals operated for a week.

It would seem, therefore, that when water is given by mouth, the first cause of the deficiency in diuresis is a prompt reduction in intestinal absorption, after which there appears more slowly some deficiency which inhibits excretion. The nature of the failure in the excretory mechanism has not been studied, and may be due in part to extrarenal factors; but that renal deficiencies do occur after adrenalectomy is well-known,[6,7] and an increased water reabsorption has been reported.[7]

CHANGES IN THE CIRCULATION.—Intoxicating doses of water do not induce in either adrenalectomized or normal animals a significant fall in blood pressure, such as follows most other stresses after adrenalectomy. Rises are sometimes seen, but not invariably. Peripheral cutaneous circulation (observed routinely in the interdigital web when taking blood pressure) was well maintained. Pressure was initially lower in adrenalectomized than in normal animals, accounting in part for the lack of diuresis, but not for the terminal collapse.

CHANGES IN BLOOD AND URINE CONSTITUENTS.——Water given in intoxicating doses causes the external loss of much chloride (circa 80 mg. NaCl) in urine and diarrheal fluid of normal rats. In adrenalectomized animals less is excreted because of the retarded diuresis, but the plasma stores are depleted by a large shift of chloride to the unabsorbed fluid of the gut.

In other species, the introduction of water induces a sharp drop in hematocrit levels,[1,8] but, in the rat, rises in hematocrit always were associated with intoxication symptoms. These were more severe in adrenalectomized (+17%) than in normal (+10%) animals. Hemoglobin tended to follow the hematocrit in adrenalectomized rats, but with a lesser percentage rise.

Plasma protein, on the other hand, fell in all cases, but much more in adrenalectomized (—27%) than in normal rats (—13%).

Plasma chloride was reduced in all cases after either diuretic or intoxicating doses. In intoxicated states, it was reduced by —21 per cent in adrenalectomized rats; but —35 per cent in normal rats.

INTERPRETATIONS.—The experimental facts listed above are not in themselves sufficient to be self-explanatory. Tentatively, however, certain inferences can be drawn which must await experimental confirmation.

It is suggested, following others,[1,9] that the primary cause of water intoxication is the loss of extracellular electrolyte (NaCl) and the consequent alterations in water distribution. When the depleted Cl level is restored and tonicity raised by injections of hypertonic NaCl, recovery from symptoms is prompt and dramatic.[1,10] We have found that even the use of 1 cc. of normal saline per hour extends life and prevents the hematocrit rise.

Calculations of chloride balance made from determinations of plasma levels, urine (and diarrheal) loss, and the shift of unabsorbed water of the intestines, indicate that the entire extracellular loss can be accounted for without the assumption of any dilution or expansion of the extracellular fluid volume.

There is no reason to postulate electrolyte shifts to the liver or elsewhere, as seems to be necessary in the dog.[11]

If, then, the extracellular fluid volume does not increase, the large amount of absorbed-unexcreted water must be inside of cells, or in some special unidentified fluid reservoir. This presumably occasions cell swelling, in which the erythrocytes share, and hence somewhat falsely high hematocrit values are obtained. A marked susceptibility to hemolysis was seen and is consistent with the idea of erythrocyte swelling. In other words, when water is absorbed in the later stages of water intoxication, it expands the intracellular but not the extracellular fluid because of the excessive chloride loss from the latter.

Body cells are then subjected to two abnormal circumstances: (1)

they are diluted and swollen; and (2) they are exposed to a hypotonic environmental fluid of very low NaCl content. Of the latter, we are certain; the former is inferred. A likely cause of the violent convulsions and other symptoms is the reaction of cells, particularly of the central nervous system, to these abnormal circumstances.

The rat differs from the dog, which has been more commonly studied, in that the dog loses much less chloride externally and hence the absorbed fluid would be more evenly distributed.

The adrenalectomized rat differs from normal ones in that: (1) chloride is "lost" largely to the gut instead of urine; (2) plasma protein is also lost, presumably by capillary leakage to the interstitial fluid,[12] thus disturbing the normal relations between blood and interstitial fluid; (3) there is some "sensitivity to hydration,"[13] perhaps associated with the protein loss, which causes toxic symptoms at lesser levels of fluid retention and high levels of plasma chloride.

CARBOHYDRATE METABOLISM.—Blood sugar does not change significantly as adrenalectomized animals go into water intoxication, nor are the toxic effects ameliorated by supplying extra glucose. In normal animals, there is a rise in blood sugar in terminal stages. This was shown to be due to adrenal medullary activity.

REPLACEMENT THERAPY IN ADRENALECTOMIZED RATS.—Rats operated for only one day respond readily to replacement therapy with cortical extract or crystalline adrenal hormones. 17-hydroxy-11-dehydrocorticosterone is, however, much more effective than DCA (desoxycorticosterone acetate).[14] Curiously, however, animals that have been operated for a week, even though maintained on salt, cortical extract or DCA, and given extra doses preparatory to testing, show erratic responses thereto and in many cases no benefit. Such refractoriness does not appear in the dog,[13] but may have its counterpart in the persisting renal deficiencies of well-treated Addisonnians.[6] Sex steroids have no replacement value.[15]

EFFECTS OF CORTICAL HORMONES IN INTACT ANIMALS.—Protection is afforded normal rats against otherwise lethal water intoxication by the use of large doses of cortical extract or DCA.[16]

TESTS AND ASSAY PROCEDURES BASED ON WATER DIURESIS.—Adrenal functional deficiencies apparently can be easily detected in certain experimental states[17,5] and in Addison's Disease[18] by water diuresis and intoxication tests. Such tests have also served as the basis for assay procedures for cortical hormones.[19,20]

THE THYROID IN WATER DIURESIS AND WATER INTOXICATION[21]

We have studied various factors which might be expected to change adrenal function, and thus perhaps alter the response to water. One such factor is hyperthyroidism, in which there is marked adrenal hypertrophy and an increased demand for cortical hormone.

In hyperthyroidism, both the diuretic response to water and the resistance to water intoxication was found to be strikingly increased. Some of the probable reasons for this follow:

(1) Others have found marked alterations in renal function in hyperthyroidism which might reasonably be expected to facilitate diuresis.[22,23,24]

(2) The hyperthyroid animal is able to excrete large volumes of water of a low Cl content. This conservation presumably has the same beneficial effect as the administration of NaCl.

(3) It is possible that the increased adrenal cortical activity, associated with hyperthyroidism, may exert an effect like that of the administration of cortical hormones. Other evidence, however, would be somewhat difficult to reconcile with this hypothesis.[25]

In any case, the effects of hyperthyroidism are completely nullified by adrenalectomy. Hyperthyroid-adrenalectomized rats succumb to water intoxication in the same manner as untreated-adrenalectomized ones, with the exception that the loss of body temperature is less.

Curiously, hypothyroidism produces slight, if any, increase in the susceptibility to water intoxication.

THE ANTERIOR PITUITARY IN WATER DIURESIS AND WATER INTOXICATION

We noticed in 1937 that hypophysectomized rats were as susceptible to water intoxication as adrenalectomized ones, and that this susceptibility could be prevented with cortical extract.[5]

At that time, it did not occur to us that small, non-intoxicating diuretic doses of water would not be easily eliminated, because we were impressed by the fact that hypophysectomized rats normally run a mild diabetes insipidus. However, somewhat later we had occasion to test for sensitivity to the antidiuretic effects of pitressin in hypophysectomized animals and, in doing so by the usual method, observed the rate of excretion of single doses of water (5% of body weight). It was apparent at once that these small doses of water exerted no diuretic effect.

They were, in fact, actually antidiuretic in that, if the animals were fasted and left alone, they would drink less water than we gave but would put out more urine than in our experiments. We thought, at the time, that this was likely due to some shock-like effect of the manipulation, mild though it was, in these fragile animals. Blood pressure determinations did not confirm the idea that shock was induced. Hypotension was present initially, but its severity was not increased by the administration of water.

Further observations were delayed, and, in the meantime, Walker[28] and Chen and Geiling[29] noted the same phenomena. Walker's observations were only incidental to other studies, but he suggested that the results were likely due to delayed absorption. Chen and Geiling showed that the effect was purely an anterior lobe deficiency and occurred either in the presence or absence of the posterior lobe.

In subsequent studies, we have found that there is a delay in intestinal absorption, but one apparently lesser in extent than in adrenalectomized animals. This means that the main difficulty was some excretory dysfunction that prevented elimination of absorbed fluid.

The deficiency, whatever its detailed nature may be, is apparently due primarily to hypofunction of the adrenal cortex. It can, at least, be almost completely repaired by small doses (2 cc.) of adrenalin cortical extract, or 2 mg. of DCA. It cannot be relieved at all by a crude anterior pituitary extract which restores most other functions, but is weak in adrenotrophin; nor by thyroxin in doses that restore basal metabolism to normal; nor by combinations of thyroxin and the pituitary extract.

Since the deficiency is actually worse than in adrenalectomized animals, there is likely some pituitary factor, in addition to that which works through the adrenal, also involved.

CONCLUSIONS

The hormones of the adrenal cortex, the thyroid and the anterior pituitary, through their effects on water absorption, electrolyte metabolism, permeability, renal function, etc., exert important regulatory influences on the metabolism and distribution of body water. The extent and, to some degree, the nature of such influences can be revealed with ease by subjecting animals, in which the level of these hormones is varied, to diuretic or intoxicating doses of water.

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DOCTOR BECKETT M. HOWORTH, Columbia University, New York, N.Y.: *Biological Aspects of Mountaineering.** (Illustrated by motion pictures, in color, and demonstration of climbing equipment.)

The title of this presentation is redundant in a way, as there are biological aspects to all phases of mountaineering. The persons who climb are, of course, biological, as well as all of their activities, such as sleeping, eating, and climbing. Their food is derived from biological sources, as well as their boots and clothing, and even some of their equipment. There are biological aspects to campsites, while trails abound with them, e.g., flowers, trees and shrubs, birds, insects, and animals. The montains themselves, though essentially geological, have many biological relations, such as the fossils which lie buried in them, the water and snow which they collect, and the soil which they form. Even the weather has a close relation to the flora and fauna of the mountains, as well as to their transient guests. Thus, we find whole biological cycles represented in mountains and mountaineering.

Mountains have been climbed occasionally since the beginning of history, for observation and for game hunting, but mountaineering is relatively recent in scientific interest as well as in sport. High mountains, generally, have been regarded with awe and superstition and even with reverence, by the native peoples in their vicinity, and often are so regarded even today. Nearly all mountaineering history has been crowded into the past hundred years. The first mountaineers were travelers or artists, like Whymper, who first climbed the Matterhorn, scientists like Gesner, in the 16th Century, or de Saussure, among the first to ascend Mont Blanc, the highest mountain in western Europe, or explorers like Fremont, who first climbed one of the Wind River mountains of Wyoming. These pioneers had almost no precedents in approach, route finding, technique or equipment. Their guides were not skillful mountaineers, but hardy and courageous natives familiar with the region, and knowing how to live in the mountains. The courage and ability of these early climbers is all the more remarkable when measured in terms of the knowledge and equipment of today.

Approach to the mountains may be easy, by boat, train, or paved highway, for food and equipment as well as personnel. Mountain hotels or huts, often with keepers who provide beds and meals, are common in Europe. Other mountains are difficult to approach, requiring days or even weeks of heavy backpacking, sometimes with the aid of pack animals, porters, or the aeroplane, as in the Himalaya or Alaska. Many mountains can be climbed in a day from a suitable base camp, while others may require the laying out of a series of high camps, with tents, sleeping bags, clothing, food, and fuel, in the manner of Arctic exploration. Often the problems of the establishment and maintenance of the camps are greater than those of the actual climb, and the organization and quality of the camps will determine the success or failure of the expedition. Many adjustments must be made for altitude and temperature, due, for example, to changes in the palatability, or boiling point of food. Reduced oxygen may result in loss of appetite or change in taste, greater susceptibility to cold, sleeplessness, greatly diminished capacity for work. Camp equipment must be chosen with great care, portability, durability, ease of operation and light weight being prime considerations.

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* This address was delivered at the meeting of the Section of Biology, February 14, 1944.

Selection of a route up the mountain may be of great importance. Some mountains may be climbed from any angle without difficulty or danger. Other mountains, such as Kanchenjunga, may have no safe route, none sure to "go." The margin of safety becomes less with increasing altitude, and the difficulty of climbing greater. Ridges are apt to be safer than faces, whereas gullies or "couloirs" are often subject to falling rocks, ice or snow. Ridges are more exposed in wind or storm. A glacier, with its numerous crevasses and ice falls, may offer a dangerous or difficult approach, yet be the only feasible one. Routes and high campsites must be selected so as to avoid avalanche paths. Maps, reports, observation with field glasses and from neighboring peaks or by air, and trial of prospective routes, all will aid in the choice of the better one. The simplest and safest route is usually chosen for an unclimbed or very difficult peak. A more "interesting" or difficult or new route may be preferred on the mountain which has been frequently climbed. Snow and ice conditions, weather, size and strength of party, time, and importance of the climb, are factors in the final choice of the route.

The basis of climbing is to get up the mountain, and not only the route, but the climbing technique are adjusted to this end. Climbing consists essentially of walking, the legs nearly always doing the major part of the work. The hands and arms are used chiefly for balance and for assisting the legs when necessary, but occasionally may do the major work for a short distance. No one can climb a smooth vertical wall.

There must be "holds" of some sort, cracks, ledges, flakes, knobs, or even large crystals of rock serving the purpose. The holds do not have to be large, as they are generally used only for a moment in passing. The climber usually uses fingers and toes, and keeps the body away from the rock. The beginner tends to choose level platforms large enough to sit or stand upon with both feet, and to remain there, whereas the experienced climber requires only something large enough for the corner of a boot or sneaker, a finger or two, and uses it only for a fleeting support. Climbing signifies motion, not parking, and the good climber moves almost continuously, with rhythm, smoothness, balance and momentum. He climbs with movements like a cat or a modern dancer. Occasionally, in a wide crack or chimney, the ankle, elbow, or knee may be used with a sideways pressure or twist to secure enough friction for a hold, or the hands may pull in opposite directions against the sides of the crack, or the back and one foot may press against one wall, while the hands and other foot press against the opposite wall. It is easy to produce enough friction to support the climber, but more difficult to keep moving upward. Crack and chimney climbing are usually more difficult, technically or physically, than climbing faces or ridges. Snow slopes may be soft, making it difficult to find firm footing, or loose, with danger of avalanching. Other snow slopes may require the kicking or cutting of steps, often a long and laborious process, but snow climbing is usually not difficult. However, deep snow along the crest of a broken, narrow ridge may be a serious problem. Ice is often quite difficult, as it is smooth, cold and slippery, may break loose, and has few natural holds. Step cutting in ice is also difficult and very tiring, especially in awkward situations.

The steepness, height and airiness of a route are usually spoken of as "exposure." For example, an overhanging rock wall with two thousand feet of space vertically below would be called "exposed," even if the climbing were not difficult. Exposure is often the most difficult part of climbing. Many people feel uneasy on going to the edge of a cliff or high balcony, and looking over. Exposure in climbing is less noticeable, however. Starting from the bottom, one gets used to the height gradually. There is more for the eyes and hands to rest upon, so that balance is easier and more secure. The rock has a solid feeling, a part of the earth, and there is more of it above. Mountain topography also has a solid, earthly appearance, and gives the eye and the balance many opportunities for perspective, as well as the distractions of beautiful scenery. The act of climbing, and the team work required, especially in the difficult parts, or "pitches," leave little opportunity for fear. Lack of fear is essentially a feeling of security, born of a knowledge of the solidity of the rock and one's own capacity. Experienced climbers, at the beginning of the season, start gradually, until they have recovered their sense of balance and co-ordination, and their "feel" of the rocks.

Rock varies considerably in texture and solidity. Granite, gneiss, and schist are hard and strong and usually dependable. The rock may fracture, largely by the action of ice, and erosion occurs. But fracture and erosion often result in hand- and footholds on otherwise unclimbable rock. Sedimentary rock is often crumbly and unsound. Horizontal stratification usually results in ledges which offer good holds, whereas vertical stratification, splitting or faulting result in cracks and chimneys which may be climbed. The climber must judge the soundness of the rock in general in choosing his route, and of each hold before his weight is put upon it, by inspection, by pulling, pushing, or even pounding upon it. It is particularly important that footholds be inspected at eye level, as they carry most of the weight and are more difficult to judge after the eye and hand have passed them. The climber must consider not only his own security, but also the safety of those who are or might be below him, should he dislodge a stone. Sometimes, the climber is subjected to the unnecessary danger of stones dislodged or thrown by people above. The mountain tourist or walker, when tempted to send down stones, should remember the possibility that there may be a climber beneath him, and that a little stone may start others downward and that even a little stone falling some distance may kill.

Certain equipment aids the mountaineer. His boots are most important. They should be broad-toed and thick-soled, strong and comfortable, large enough for two pairs of heavy socks, but not loose. Hobnails provide a better grip, especially on ice, snow, and wet surfaces. Edge nails are useful around the edges of the soles and heels. Tricouni nails are harder and may grip better on slippery grass or hard snow. Crampons with long spikes may be attached to the boots for a still better grip on hard snow and ice, but may be dangerous if used carelessly or clumsily.

Clothing should, for the most part, be woolen, in several layers, with an outer garment, such as a parka of wind- and shower-proof poplin or some similar closely woven material.

A rope is used as a safety device when its use would reduce the danger to one of the party. The rope is usually a long-strand, selected manila or hemp, about 130 feet long and one-half inch thick, capable of supporting a ton of dead weight. It is tied to the waist of the leader by a knot such as a bowline, which will not slip. The leader is not prevented by the rope from falling, but will not fall so far. He should climb as carefully as though he did not have the rope. When he reaches a secure stand, he braces himself in a special way, or ties himself to a tree or rock, takes up slack, and "brings up" or "belays" the next man, who has the middle or end of the rope around his waist. The leader has passed the rope over his shoulder or hip, and takes up slack as the next man ascends, so that if the climber came off he would not fall, but be held by the rope. Climbing can thus be made completely safe for all but the leader with proper rope technique. Should the leader reach a point where his margin of safety is too small, he may drive into a crack in the rock at piton, a malleable iron spike shaped like the letter "P." He snaps a carabiner, a ring with a hinged side, through the ring of the piton, slips the rope through the carabiner, and thus reduces the rope length between himself and his nearest support in case of a fall. The piton may also be used for "tying in" at belay points. The rope and its accessories may be used in other ways, but there is not space to discuss them all here. Roped climbing requires a careful and precise technique, considerable experience, and meticulous teamwork, but, when practised in this way, can add greatly to the morale, safety, and climbing capacity of the party.

The ice ax is of great value in snow and ice climbing. It has a pick and an adze blade on the head, and a point on the end of the handle. It may be used for balance and support, like a cane; it may be plunged into the snow as a support for the climber or for the rope, similar to the use of piton and carabiner; it may be used for cutting steps in snow or ice; and it may be used as a brake in sliding down a snow slope on the feet ("glissading," similar to skiing without skis). There are special ice pitons for use in ice climbing. Skis may be used in snow-climbing, and add greatly to the speed and pleasure of the descent, as well as to the interest of the ascent.

The dangers of climbing are not great, and can largely be avoided by a proper choice of route and by care in not knocking off stones.

Avalanches can usually be avoided by proper route selection, and by knowledge and experience. Storms and exposure to wind, cold, and weather, constitute the greatest danger in high mountains. They cannot be avoided or predicted, but, with proper clothing, equipment, camps, and conditioning, their dangers can be minimized. Mountaineering is about as safe as riding in automobiles, and usually need not have the hazards of the unpredictable other party.

Mountaineers are invariably asked, "Why do you climb?" The simple and truthful answer, "Because I like it," is the usual one. The answer of the great English climber, Mallory, who was lost on Everest, is perhaps most expressive to the mountaineer—"Because they are there." Don Blanding, in his poem "To Don May—who climbed mountains with me," has expressed it most completely. The reasons for climbing vary with different people, different circumstances, and at different times. The scientist is interested in the observation of geology, of glaciers, of weather, flora and fauna. The nature-lover is attracted by all these things for their beauty, as well as their construction and activity. The mountain landscape, and mountain storms, sunrises, and clouds, offer some of the Earth's finest scenery, to the delight of viewer and photographer alike. The mountains have furnished inspiration for fine literary passages, both poetry and prose, and spiritual satisfaction since Biblical times, to many people. There is physical joy and mental stimulation in climbing, especially when it is interesting and difficult. It offers physical and mental change and relaxation at its best. The adult is peculiarly fitted to the slower, steadier pace of mountaineering, as compared with more violent sports. The associated camplife, fishing, riding and packing have their joys. Best of all are the companionship, mutual effort, and quiet understanding of those who climb together, revealing, in the vastness and magnificence of some of nature's greatest creations, much of the fineness and glory of man.

SECTION OF ANTHROPOLOGY

March 27, 1944

Doctor Joseph Bram, Queens College, Flushing, N. Y.: *The Social Identity of the Jews.*

Countless attempts to place the Jews in clear sociological categories have still left them an undefined social phenomenon. Louis Wirth has said that "the elementary question as to whether the Jews are a race, a nationality or a cultural group remains unsettled." Other writers who have tried to find a proper definition for them have expressed the difficulties they encountered in very characteristic terms. They refer to the Jews as "an unusual type of nationality," "a social anomaly," "a peculiar people." Talcott Parsons considers them "a unique social phenomenon," Carl Mayer calls them "a chimeric people" leading "a life of unreality" . . .

One must not think, I believe, that only professional sociologists and anthropologists are aware of the existence of social groups designated by different terms. Taxonomic concepts used popularly with reference to social and ethnic groups may be less refined and less consistent than those coined by social scientists. They are likely, however, to carry more weight in the sphere of political and social action. In so far as men act upon their beliefs, the concepts and convictions of unscholarly men, unfounded as they may be in the eyes of trained social scientists, are of great influence in determining social attitudes and social behavior.

This is why, I believe, it would be of value to survey the typical ways in which the Jews have identified themselves as a social phenomenon, as well as the popular interpretations of them made by the surrounding world. I submit that this survey would show that the social identity of the Jews, both as understood by themselves and as seen by others, becomes a factor of importance in determining the relationships between the Jews and the peoples among whom they live.

I will first deal with the diverse self-identifications current among the Jews.

For a *pious orthodox Jew* the nature of the Jewish people has been defined once and forever by the Old Testament. Chosen by God as the instrument of his will, Israel has a mission to perform. The destiny of the Jews is determined by the Covenant between them and the

Almighty. Those who profess this view interpret the sufferings that the Jews endure and the persecutions visited upon them as the expression of God's wrath.

A *religious traditionalist,* who does not have strong mystico-historical leanings, also accepts religion as the principal feature of Jewishness. He is, however, more likely to emphasize the rituals and the ethical values of Judaism rather than the exclusive supernatural rôle conferred by God upon the Jewish people. Many representatives of this viewpoint think of the Jews as a strictly denominational group similar to the Presbyterians, the Greek Orthodox or the Mohammedans, and repudiate all interest in the non-religious activities of the Jews as a people (e.g., the creation of Jewish Palestine).

For a *cultural autonomist* the Jews are not only a religious group but also the carriers of a broad cultural heritage which embraces their languages (Hebrew and Yiddish), literature, folklore, music and other elements which they consider worthy of preservation and perpetuation. Cultural autonomists oppose the levelling effect that cultural monism has in any particular nation. They believe that genuine democracy should provide for cultural diversity

A *Zionist* is not necessarily a religious Jew, although many undoubtedly are. A Zionist views the Jews as a nation without a homeland, a people in exile. From his viewpoint, religion has served the function of an enclosure which has prevented the Jews from losing their national identity. With the declining influence of religion, however, the Jews can survive as a nation only if they regain their ancestral land —Palestine. The Zionists consider gradual cultural assimilation with the non-Jewish world a very painful, undesirable and all too frequently unsuccessful process.

The so-called *assimilationists* consider Jewish traditions and the Jewish cultural heritage valuable and respectable in terms of historical perspective, but of relatively limited scope when compared to the achievements of modern Western civilization. They believe that the Jewish tendency to cling to the past only postpones the inevitable, and to them desirable, disappearance of the Jews—their dissolution in the surrounding world. They condemn any planned and organized perpetuation of Jewish culture and frequently of Jewish religion as well. In so far as being Jewish is associated with social discrimination and persecution, Jewishness becomes for some assimilationists more than just a cultural anachronism, and assumes all the characteristics of a social disease, i.e., something to be minimized or even concealed altogether.

It would be a mistake to ignore the existence of *Jewish racialists,* who believe that the Jewish people, due to millenia of endogamy, represents an unusually pure racial strain with inheritable physical and psychological characteristics. Jewish racialists may hold varying views with regard to Zionism, the Jewish religion or problems of cultural autonomy, but they firmly believe in the reality of the inherited national Jewish character which manifests itself in whatever field of endeavor individual Jews choose to apply their talents. Jewish racialists believe that Spinoza, Marx, Disraeli, Freud and Einstein are specific Jewish geniuses and that the voice of the "racial spirit" is audible in their work. They also believe that the Jewish personality type maintains itself through the ages regardless of diverse environmental influences.

These six views outlined here in reality overlap and combine with each other to varying degrees. They are not always expressed in articulate and consistent terms, and, on the whole, it can be said that great disorientation prevails among the Jews as to their own social identity.

If we turn, now, to the Gentile world, we find there the same lack of agreement regarding the social nature of the Jewish people.

The traditional *Churches of Christianity* view the Jews in the light of the Christian conception of history. They recognize them as a people chosen by God and castigated by him for its failure to accept Christ. From an orthodox Christian viewpoint, the Jews are a "metaphysical" people, the living proof of the authenticity of the Scriptures.

The *less literal-minded Christians* emphasize the religious distinctness of the Jews as the only non-Christian group admitted to participate intimately in the life of the Christian society but do not stress their unique rôle in the cosmic historical drama.

Where the Jews were classified primarily as a religious community, they were treated by the surrounding society in accordance with the degree of religious tolerance prevalent in that society. Thus, they were persecuted in old Russia as a religious minority. Individual Jews, however, could always escape discrimination and persecution by becoming converted to the religion of the majority, since only religion was accepted as a criterion of their Jewishness.

The French, too, defined the Jews as a religious group but treated them in keeping with the official ideology of the Third Republic, which considered religion a matter of individual choice.

The tendency to consider the *Jews as a race* or a sub-race rather than a religious or cultural minority has been gathering strength since the end of the last century. This tendency is correlated with the specific observable characteristics of the Jews as a minority group which are in contrast with those of the majority of the particular nation in which they live. In this context, "race" is not to be understood in terms of predominant measurable physical characteristics typical of the whole group, but as a visual image shared by the members of a particular society which enables them to identify certain individuals as belonging to what is believed to be a separate racial stock.

This identification is based only partially on physical characters which may be more frequent among the Jews than among the non-Jews in a particular nation. To a far greater extent, the apparent physical identifiability of the Jews is based on cultural characteristics such as gestures, facial expression, speech mannerisms, food habits, clothes, occupations, religious practices and other non-inheritable non-biological features associated with them.

Once the members of the non-Jewish majority have accepted this complex of Jewish earmarks as indicating racial distinctness of the Jews, the usual anthropological argument which denies objective reality to the concept of a Jewish race meets with scepticism. Too many individuals believe that they can identify the Jews on the basis of what they consider to be inherited physical characteristics. . . .

Physical anthropologists should understand that they are not dealing simply with people misinformed on the question of Jewish racial uniformity, but are confronted by popular racial ideologies, which operate with their own implicit definitions of race, their own criteria of classification and their own beliefs regarding the inheritance of learned behavior—a complex of convictions impermeable to isolated data. Before attempting to substitute their objective truth for the generally accepted and functioning "truth" in race-minded nations, anthropologists should analyze more closely the nature of popular fallacies and misconceptions and the reasons why they persist.

We have, thus far, dealt with two interpretations of the Jews by the outside world—the religious and the racial.

There are non-Jews, however, who are more sensitive to the cultural duality of the Jews than to their religious and seeming racial distinctness. The continued use of Yiddish and Hebrew by many Jews, along with various customs and folkways, is sometimes viewed by the non-Jews as a proof of the Jewish inability or unwillingness to assimilate. Others, who accept the ideals of cultural pluralism, welcome the specific manifestations of Jewish culture as contributing to the broadening and enrichment of the total culture of the larger society.

There are non-Jews who believe that the Jews form a real political national minority with interests not limited to the country of their residence. The creation of Jewish Palestine and the existence of international Jewish organizations of relief are pointed to as indications of dual political allegiance.

The various Jewish self-identifications and the ways in which the Jews are identified by the peoples among whom they live influence each other. Thus, in a race-minded society, the Jews become more keenly aware of their racial distinctness, while, in a society that emphasizes religious criteria for group delimitation, they are likely to consider themselves as primarily a religious community. On the other hand, a Jewish community that actively cultivates its own values (through education, the press, etc.) may lead the surrounding society to consider its status, not in terms of the traditional frame of reference of religion and race, but in terms of a conflict between the ideals of cultural pluralism and those of cultural monism.

Why has it been so difficult to establish the social identity of the Jews in universally acceptable terms? Several tentative explanations can be offered. In the first place, most discussions of the Jewish identity assume, by implication, that the Jews are a relatively static people. Their past is used as a point of reference for the interpretation of their present. Since, in the past, the Jews were a uniformly religious group, religion becomes the principal criterion of Jewishness, the process of Jewish secularization is ignored and the Jews are defined as "ein Volk der Religion."

Another reason for disagreement regarding the social identity of the Jews is the common tendency to judge the whole by its parts. The Jewish people is too often defined on the basis of local and regional sampling. The characteristics of any one sub-group are ascribed to the entire people and its essential ethnic and cultural heterogeneity is overlooked. One easily forgets the black Jews of Ethiopia, the brown Jews of Cochin in India, the old Jewish communities of China, the orientalized Jews of Saudi-Arabia, Yemen, Iraq, Iran, Afghanistan, the Jewish mountaineers of the Caucasus, and many other non-Western Jewish groups.

Also, in defining the Jews in the Western world, too many writers have remained oblivious to the differential participation of individual Jews in the complex of Jewish characteristics. The knowledge of Hebrew and of Yiddish, the participation in the Jewish religious practices, the familiarity of the Jewish past and the interest in the Jewish communal life are distributed very unevenly among individual Jews. A field-worker could easily find Jewish groups leading a fully Jewish existence, eating Jewish food, singing Jewish songs, and reading Jewish books. Jewishness in such circles is accepted as a familiar, natural and desirable way of life.

At the other end of the scale, the same field-worker would find just as many people for whom Jewishness is associated with vague family reminiscences and less vague frustrations and anxieties caused by social discrimination, but devoid of any linguistic, cultural and religious content.

The two extreme groups, those who want to be Jewish and those who cannot escape being Jewish, have, for a common denominator, only common ancestry and the recurrent hostility of the surrounding world.

This discussion of the social identity of the Jews has been inspired by the desire to plead the case of a realistic functionalist approach to the problems of social and ethnic identity. Instead of trying to define the Jews in terms of preconceived taxonomic criteria only, I have attempted also to explore the many ways in which the Jewish groups identify themselves and are viewed by others. I have also tried to show that there is an interacting relationship between these two sets of popular concepts.

The picture of the Jewish people that emerges out of this discussion is one of extreme heterogeneity. It is also a picture of a changing people interacting with diverse national societies and whose social identity is constantly subjected by both sides to fluctuating interpretations.

NEW MEMBERS

Elected March 23, 1944

ACTIVE MEMBERS

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Dyk, Walter, Ph.D., Anthropology, Instructor in Anthropology, Brooklyn College, Brooklyn, N. Y.

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Johnson, Frank H., Ph.D., Biological Rate Control and Action of Inhibitors, Assistant Professor of Biology, Princeton University, Princeton, N. J.

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY

APRIL 3, 1944

DOCTOR HARLAN T. STETSON, Massachusetts Institute of Technology, Needham, Mass.: *Modern Evidences for Differential Movement of Certain Points on the Earth's Surface.* (This lecture was illustrated by lantern slides.)

The subject of our discussion is one of those problems that not only straddles the border line between departments of knowledge, but transgresses far into the adjacent territories of several sciences.

Making no pose as a geologist, however, I must confess that what I may have to say in regard to physiographic examples of differential displacements in the earth's crust will have to be from such little knowledge as I have been able to borrow from geologists, and I venture to proffer such second-hand information only on the ground that we have a board of experts among us to whom questionable points may be referred subsequently. My excuse for venturing upon this subject comes from a very vital interest that must concern every student of the earth.

Certain investigations, with which I have been more or less engaged for the last fifteen years, seem to lend promise that the problem of lateral shifts in the earth's crust may be helped toward solution by the continued observation of the latitude and longitude of certain presumably fixed points on the earth's surface. In the American Geophysical Union, that unique organization of students of the earth-sciences, ex-

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Executive Secretary Eunice Thomas Miner.

Assistant Editor · Ethel J. Timonier.

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perts from many different but interrelated fields commingle freely. It would appear that it was through such commingling that some astronomers became contaminated by some geologists, perhaps with a fond hope that a little contamination of astronomy with geology might be of mutual benefit.

The most dramatic evidence for translational displacements in the earth's crust are still to be found in the geologists' picture album, and I am indebted to our colleague, Professor Thom, for the loan of certain photographs that portray, graphically, displacements of many feet along fault lines familiar to every geologist. Not all of these displacements are sudden occurrences, such as accompany great seismic disturbances. The slow crawling of the crust in the neighborhood of the San Andreas fault is well known to the petroleum industries, whose pipe-lines have bent and buckled and often broken under the stresses and strains of the earth's crust. I have inquired of my geological friends for evidence in other parts of the country of equally striking displacements,—thus far without very encouraging results. It would appear that California, in keeping with her other superlatives, may continue to boast not only as the state par excellence of Hollywood stars and gigantic telescopes, but also of maximum terrestrial displacements.

Of course, looking back into geologic history, we have a panorama presented of a long series of depressions and upheavals of overthrusts and underthrusts which have caused the characteristic formations of the orogenic zones. Dr. Bucher has stressed the role of thrust-sheets in orogenic deformation and would cite as a specific example the Cumberland thrust-block lying across the tri-state corner of Kentucky, Virginia, and Tennessee.[1] Here, a slice of rock over 120 miles long, some 20 miles wide, and a mile in thickness, has been sheared off closely parallel with the bedding, and pushed along a distance of at least 7 miles. In addition, the whole slice has apparently been rotated, producing cross wrinkles.

The presence of great rift or shift zones in continental shield-areas was graphically depicted by Dr. A. W. Jolliffe,[2] at a recent symposium, as follows:

> "Many faults in the Canadian Shield are known to follow essentially rectilinear vertical fractures that extend for distances up to several hundreds of miles. None of them is known to be marked by major vertical displacements whereas a number show strike-slip movements amounting to several miles. From this it is inferred that the fault-movements were dominantly horizontal of the same general

nature as those along the San Andreas Rift. Movements along many of the Shield faults were recurrent, the most recent being late Pre-Cambrian or even younger in age."

While geologists generally recognize that large differential horizontal displacements have taken place in the past, probably many still believe that such movements between crustal segments are negligible at the present time. Professor Thom,[3] in a recent report of his special committee on the Geophysical and Geological study of continents, stresses that "Evidence has now accumulated to a point where it is reasonably clear that an appreciable differential crustal shift is still going on in different parts of the world. And it, therefore, seems highly expedient that adequate data as to nature, magnitude and disposition of these relative movements should be obtained forthwith because of the critical bearing of these data upon over-all geodetic and cartographic programs and operations."

It seems to be beyond contention that a certain recurrence, at more or less definite intervals, or a cyclical rhythm in large crustal displacements, has taken place in geologic time. Accumulating evidence continues to point that such movements must still be taking place; but when large masses of terrain are involved, especially in regions unscarred by fault lines, there is a difficult problem in determining either the direction or the amount of such movements as may be taking place currently. It is for this reason that the geologists look to the astronomers to see what evidence there is, if any, that displacements have taken place in very recent times. To interweave more closely the determination of the crustal movements with the astronomical method of finding changes in position, we perhaps should remove ourselves sufficiently far from the earth itself to get the cosmic pictures of the planet with which we are concerned, and its immediate environment.

To use an analogy, those of us who inhabit the earth find ourselves aboard a ship on the high seas of space Contrary to the opinions of the earlier astronomers, this spherical ship is far from fixed. At this very moment, we are being carried eastward with the earth's daily rotation at a velocity, in this latitude, of about 600 miles an hour. Meanwhile, the good ship Earth is steaming ahead in its orbit about the sun with a velocity of 20 miles a second. The sun, however, which is the flagship of our fleet of planets, is steaming ahead on its course among the nearer stars at a velocity of some 12 miles a second, or 40,000 miles an hour. The sun, in company with other suns comprising a still larger squadron, is, in turn, sailing through space at a speed of several hundred miles a second toward the lights of other celestial ships seen at the limit of the horizon of our greatest telescopes. Meanwhile, as passengers aboard the earth, we are being tossed to and fro, all quite unconsciously, due to certain peculiar wobblings of the earth's axis, and now even the deck of the ship on which we stand—the crust of the earth—proves itself unstable as it trembles now and then, due to internal catastrophes within the ship's hold.

As passengers aboard our frail celestial craft, we are subject to all the cosmic forces, known and unknown, that play about in the mysterious ocean of space. Only as we examine our environment, can we hope to come to anything like a complete knowledge of some of the disturbing forces that are operating upon the earth. It is to astronomers, as the navigators upon the bridge of this strange ship, that our fellow passengers look to tell the ship's position, define her course, and speed. Only by observations of those lights exterior to the earth—the distant stars—can we hope to evaluate not only the motions of the earth but movements that may be taking place in the earth's crust. While, in the last analysis, we can never hope to define the absolute motion of ourselves among a vast system of moving stars, we can take courage at the degree of precision which may be attained from astronomical observations. For those not intimately acquainted with the applications of practical astronomy, it comes perhaps as something of a surprise when we consider that it is possible, by looking at the stars, to determine one's position on the surface of the earth with an uncertainty of but a very few feet. More specifically, from a series of careful observations, we can determine, within a foot or two, where we are north and south (latitude) and, in the east and west direction (longitude), within an uncertainty of perhaps three or four times that amount.

The problem of the determination of latitude is fundamentally very simple, since an observer is as far north of the earth's equator as his zenith appears from the celestial equator, the zenith being determined by the direction of the plumb-line at a given point. One needs only to attach a telescope, in a sufficiently expert manner, to a level bubble and the zenith is located among the stars, whose positions have been catalogued with respect to the celestial equator. Unfortunately, however, the positions of the stars can be determined only if the latitude of the observatory is known; so that it is only through a series of successive approximations of a large number of cooperating observatories that the positions of the stars can be known so that the latitude of any station can be absolutely determined.

VARIATIONS IN LATITUDE

For finding small variations in latitude at a single station, the true value of the star positions is not necessary, provided the star has the courtesy to remain stationary during any series of latitude observations in which we are interested. From a long series of observations made at a selected number of stations operated through international cooperation along the 39th parallel, it has been possible to evaluate, with great accuracy, small variations in latitude. It has been determined that the pole of rotation of the earth is constantly shifting within the earth itself. This wandering of the pole, first announced by Chandler and later by Kustner of Berlin, is of a somewhat cyclical nature, having a period of approximately 428 days, with a superimposed annual component. The migration of the earth's pole of rotation about the pole of its figure has never yet been observed to exceed a radius of 30 feet. An inter-comparison, however, from time to time, of the latitudes of the several stations involved, namely, Gaithersburg, Carloforte, Chardjui, and Mizusawa, have revealed certain discrepancies that may well have indicated crustal displacements. In the reduction of the last International Latitude Survey, Kimura[4] bluntly stated that, with the position of the pole determined from all the other latitude stations excluding Mizusawa, the latter station could be considered to have moved southward by about 3 meters from the position occupied ten years earlier. On the other hand, certain geodesists are loath to accept this as evidence, proferring rather the alternative interpretation that the discrepancy lies in errors in the adopted position of the stars used in the reduction of the observations. With a certain daring impishness, however, I venture to remark in paraphrase "The trouble, dear Brutus, may not be with our stars, but with ourselves, that we are moving."

It should be emphasized that, in determining latitude, we are concerned with the precise location of the zenith among the stars. Anything that influences the direction of the plumb-line, peculiar to the observatory station, will show up as a variation in latitude. A shore station, subject to the loading of the ocean tides, will have its plumb-line periodically deflected in the course of a lunar day, by the gravitational attraction of the rising tide of water off shore. The tilting of the land which results from the hydrostatic pressures will be readily manifested on a tilt-meter, but this tilt will not displace the direction of the plumb-line, nor cause a variation in latitude, unless there is a lateral shift along the equipotential surface. The distortion of the plumb-line due to the attraction of a mass of ocean water off shore has been worked out and can not much exceed one one-hundredth of a second of arc. A tilt of the coast line, due to the loading, may amount to a considerable fraction of a second of arc for some distance back of the shore-line itself.

The moon, of course, causes a calculable tide in the solid earth as well as in the oceans. The celebrated Michelson-Gale experiments, carried out on the ground of the Yerkes Observatory in 1913 to 1915, made possible the observation of well-defined earth tides through a tilt-meter consisting of a horizontal pipe 500 feet long, half filled with water. The microscopic tides caused in this pipe were measured by an elaborate optical method, and revealed amplitudes in the waves less than those calculable on the assumption of an unyielding earth. The discrepancies between the observed and the calculated values of the tides made possible the evaluation of the amount of yield in the solid earth. From the results, it was found that the rigidity of the earth, as a whole, was perhaps a little greater than that of steel.

It should be pointed out in this connection that such a calculation must, of necessity, be made for the earth as a whole as a homogeneous body. Were there a larger yield in the crust than in the earth's interior there would be no way of determining it by this method. Again, it should be remembered that the observations revealed tilts only. Were there an actual rhythmic translation of the crust, as the tidal wave passed the instrument, this could not have been detected by the water pipe. On the other hand, any horizontal displacement or movement of the superficial crust along the equipotential surface under the influence of the tidal force would cause the plumb-line to shift its position, and a variation of latitude would be detected by the zenith telescope.

It was in the interest of trying to discover whether or not such tangential displacements as might take place could be detected by astronomical observation that I spent some years in investigating latitude observations for any variation that might appear to correlate with the moon's position. The results of the investigations indicated what appeared to be a definite lunar correlation in the Gaithersburg series, made with a photographic zenith tube in 1913–1914.[5] Further studies revealed similar correlations for the observations made at Ukiah. The amplitude of the variations found were of the order of several hundredths of a second of arc, corresponding to lateral shifts from two to five feet in the north and south direction.

These results were considerably larger than those that would be anticipated on the assumption of an earth tide calculated theoretically for the earth as a whole. Others, who have made a rigid harmonic analysis of latitude observations for a lunar tide, found results consistent with the theoretically calculated earth tide. It should be remarked, however, that, in my study, the form of the curve of latitude with the altitude, or time of lunar day, has by no means repeated itself completely in successive years. Patterns differ both in phase angle and in amplitude. Such results would not be inconsistent with tidal forces encountering resistance to deformation. Such an effect could be completely masked when several years of observation are subjected to a rigid analysis that pre-supposes symmetric harmonics.

It is of particular geophysical interest to note that the stations conveniently selected by the International Union for determining latitudes happened, with one or two exceptions, to lie in orogenic zones. The stations originally selected were Gaithersburg, Maryland; Cincinnati, Ohio; Ukiah, California; Mizusawa, Japan; Chardjui in the Himalayas; and Caloforte, off Sardinia.

Observations at the Cincinnati observatory were early abandoned because of discrepancies and large probable errors in the results. The Gaithersburg observatory was next abandoned for a time, but, after a lapse of several years, has again, fortunately, been reoccupied. The location of the other stations are, without exception, in extraordinarily interesting sections geologically, and perhaps could not have been better placed if the astronomers had been hunting for discrepancies that could be caused locally through crustal displacements. The astronomers, on the other hand, are naturally reluctant to attribute latitude discrepancies among the several stations in question to differential displacements. The object of the latitude project was patently to observe the motion of the earth's pole of rotation, rather than to try to discover crustal movements in the neighborhood of the telescope, which they presumably believed fixed on concrete piers where they were intended to stay put.

I make one further comment on this problem of latitude before discussing the East and West coordinate of longitude. Some twenty years ago, Dr. Lambert, of the Coast Survey, presented an exhaustive discussion of the latitude observations at Ukiah, California,[6] in which he showed that, during the decade and a half from 1900 to 1915, the latitude of Ukiah had more or less steadily increased some 0.15″. For some reason, it seems easier for astronomers to allow the imaginary earth's pole to slip within the crust than to assume that the actual territory surrounding Ukiah had, during this interval, inconsiderately slipped north 15 feet; so the interpretation of the result at first was that the North pole of the earth had been slowly migrating toward North America. This did not appear plausible, so it became more or less the consensus of opinion that the trouble, after all, was with the stars—that they were drifting. By arbitrarily correcting the declination of the stars used for observation, one could make the earth's North Pole behave more seemly, and thus not excite the Californians over an unwarranted skid toward the North Pole. That, at least, placed the chief worry in the hands of the astronomers, and relieved the geologists and real estate agents of any cause for alarm. Who, other than the astronomers, cares especially whether the stars are drifting or not? We can keep that secret within the family. However, more seriously, even some astronomers, in off moments, have geological interests, and it is coming to be recognized that, while it is difficult to prove, or disprove, some translational movements in the region of the latitude stations, some of us are advisedly considering other methods of observation that do not pre-suppose a knowledge of the absolute declination of stars, that, in turn, must of necessity be found only from assuming values for the latitudes of the observatories from which the observations are made! The only apparent method of accomplishing this is the observation of stars above and below the pole. This can be well done in high latitudes. Unfortunately, on account of atmospheric refraction, one cannot expect the degree of precision in such a method as is attainable by the adopted Talcott Method of zenith observations.

The possibility of comparing latitudes of certain well-established stations after an interval of 70 or 80 years interested Dr. Frank Schlesinger, late Director of the Yale Observatory, and I quote from a letter written by him to the Director of the Coast and Geodetic Survey on May 5, 1930:

> "As you are doubtless aware, the problem of variations of latitude has recently taken on a new aspect of interest, owing to the possibility of a secular change, as

well as periodic changes, in the position of the pole. This is one reason why the resumption of observations at Gaithersburg has become so important. An independent way in which considerable light can be thrown upon this question occurred to me several years ago, and I have discussed it with a number of my colleagues, including Doctor Bowie, chief of your division of geodesy. The plan is to reoccupy for one or two nights each, a number of the stations at which observations for latitude were made 20 or more years ago by the Talcott method. The same stars should be observed now as were used then. From the results obtained in this way we may reasonably expect light to be thrown upon the following questions: (1) The actuality of a secular motion of the pole; (2) the possibility of slow continental, or at least general drift; (3) a better determination of the systematic errors in our adopted systems of proper motions of the fixed stars."

As a result of this letter, three stations in the United States, whose latitudes had been well defined, were selected for reoccupation: Mt. Pleasant in Maine, Mt. Tom in Massachusetts, and a station in Des Moines, Iowa. The latitude of these stations had been accurately determined in 1851, 1862, and 1869, respectively. In all cases, the Talcott method of the Zenith telescope had been used. The results of this reoccupation of these stations showed that, using the same stars and the best available star positions from the well-known Boss catalogue, Mt. Tom had decreased in latitude by 0.99″; Mt. Pleasant in Maine showed an increase in latitude by 0.29″; Des Moines, Iowa, showed a decrease in latitude by 1.49″.[7]

Translated into linear distances, the evidence showed that, in the last three-quarters of a century, Mt. Tom had slipped about 100 ft. south; while Mt. Pleasant in Maine had gone north about 30 ft.; and Des Moines, Iowa, indicated a southerly drift of 150 ft., with respect to the earlier position.

The Lick Observatory, Mt. Hamilton, California, is one of the few remaining major observatories that has continued positional astronomy. An analysis of the latitude of this observatory, based on meridian circle observations from 1893 to 1918, was made some years ago by Professor Lawson and published in the bulletin of the Department of Geology of the University of California.[8] Professor Lawson's discussion exhibited evidence of an increase in latitude of 0.40″ from 1893 to 1903. In 1903 a conspicuous drop of 0.63″ in the value of the latitude occurred, an interval coincident with the earthquake of August 2, 1903. The somewhat more scattered observations from 1903 to 1918 showed a subsequent increase in latitude, so that, by 1918, the position of the observatory was essentially the same as at the beginning of the series. Dr. Campbell, then Director of the Lick Observatory, while considering that small changes in latitude may have taken place at about the time of the earthquake, believed that a certain amount of the change pointed out by Professor Lawson could be accounted for by possible accidental errors. The otherwise apparent steady northward drift of the Lick Observatory is quite consistent with, though somewhat larger than, the increase in the latitude of Ukiah, and, I might also add, of Gaithersburg, which Dr. Lambert of the Coast Survey has discussed at length. During the earthquake of 1906, when dislocations of six meters were reported, no definite proof of a change in the position at Mt. Hamilton was found from Lick Observatory observations. It is surprising that, from triangulations made before and after 1903, the report was made that no certain evidence existed for a shift in the position of the Lick Observatory.

VARIATION IN LONGITUDE

Fortunately, when we come to the consideration of detecting movements of the earth's crust in an east and west direction by astronomical observations, we do not have to be concerned with the absolute position of the stars. The difference in longitude between two places is nothing but the equivalent of the amount of time that elapses between the moment when a star is seen on the meridian of an observer at one station, and the moment when it is seen on the meridian at a second station west of the first. If an observer at the Naval Observatory in Washington learns, by wireless, the exact instant when a star has been observed as it crosses the meridian of Greenwich, and finds that 5 hours, 8 minutes and 15.78 seconds have elapsed before it crosses the meridian of Washington, he knows that the longitude west of Greenwich is 5 hours, 8 minutes 15.78 seconds; or an equivalent in arc of 77° 0′ 3.7″. If, from time to time, this interval should vary in the last decimal place, he might suspect, if he is mobile minded, that the longitude between Greenwich and Washington varied.

While in the problem of differences of longitude we have delightfully dismissed the problem of accurate knowledge of star positions, we have introduced yet another pernicious uncertainty. This is the degree of accuracy with which we can call up London and find the exact moment of the meridian passage of any given star. Of course, in practice, many stars are observed on both sides of the Atlantic, and the best possible values that such observations allow are made for the corrections for the Greenwich Observatory clocks. The exact time is then broadcast at stated intervals, by radio, which can be picked up by our Washington observatory and compared with the Washington time of transit of the same stars. Meanwhile, however, we have had to rely upon some mechanism that would keep accurate time during the five-hour interval from the moment when the stars are observed in Greenwich and the time when the same stars can be observed in Washington. Fortunately, we now have rather good timepieces, which can be trusted with an astonishing degree of reliability. In the old days of longitude observations, time signals had to be passed through the Atlantic cable, and large uncertainties in the reception of time signals from England were inherent in the manner of the electric performance of the cable. Time signals can now be exchanged by radio waves, which are commonly assumed to travel with the velocity of light. Allowing for all knowable uncertainties, one may state, without exaggeration, that time can be determined on both sides of the Atlantic and signals exchanged, with a probable error of only ± 0.007 seconds.

A few years ago, Mr. Alfred Loomis and myself became interested in what appeared to be unaccountable variations in time observed on both sides of the Atlantic—variations many times larger than any errors of observation or transmission by radio would seem to allow. Such variations, furthermore, showed a more or less rhythmic oscillation over periods of days and weeks. At that time, we felt that there was fairly good evidence that the position of the moon is definitely related to small shifts in latitude. We therefore undertook to see whether or not the small differences in determination of time on both sides of the Atlantic, that might represent small variations in longitude, could also be attributable to the moon. The presentation of the first paper on this subject to the American Astronomical Society[9] at a joint meeting with the American Association for the Advancement of Science at Atlantic City, in 1932, so startled the "New York Times" that its front page, the next morning, startled the authors, the astronomers, and the rest of the world with the idea that the two English-speaking people on the two sides of the Atlantic were sometimes nearer to each other by 60 feet, and sometimes farther away from each other by 60 feet, dependent upon what the moon had to say about it. Additional investigations by Mr. Loomis and myself, and later published by the Royal Astronomical Society,[10] seemed to corroborate our earlier findings. The variation in time which appeared to correlate with the hour angle of the moon, or the hour of the lunar day, represented recurrent discrepancies of about five hundredths of a second of time. If interpreted as a lineal change of distance in the east and west direction, this would correspond to a range of 64 feet. While we were cautious in not forcing any geological interpretation as an explanation for the phenomenon, we could not but speculate as to what hypothesis could be found to explain sanely a to and fro movement of the earth crust, were such really to exist. Much water has gone over the dam since this was first published. As to this hypothesis, there have been, geologically speaking, periods of submergence and emergence, of inundation and denudation. I think I can very briefly summarize what has happened since.

I regret that it is not possible here, on account of time, to present more details of the evidence which Mr. Loomis and I presented for a correlation between the position of the moon and the suspected variation of longitude. I should emphasize that we called attention to the fact that our table showed a marked seasonal variation about which it was remarked we should have more to say later in another paper. We did, however, publish, date by date, the reduction of the time signal observations upon which our findings were based, in the hope that others, challenged by the problem presented, might be interested to make further contributions.

We did not have long to wait. A Japanese investigator, Kawasaki,[11] utilizing our tabulated data, soon took the floor and showed that the reversal in phase of our curve of longitude variations, which we had attributed to the change in the declination of the moon from the north to the south side of the equator, could be accounted for by an annual term of unknown origin; this being the seasonal variation in the values of longitude variation to which we had called attention.

We now know that Kawasaki was wholly right in showing that, under the conditions of which the star observations were made, such an annual term could produce the effect observed, so far as the reversal of phase in the sine curve was concerned. He made no attempt, however, to account for the occurrence of the annual term or seasonal variations, or to deny that such an effect could be produced by the moon.

Shortly after the publication of Kawasaki's paper, Professor Schlesinger, after correspondence with me, undertook to make the necessary calculations to see if the known variation in the position of the pole, that caused the annual term in the variation of latitude, could not introduce an appreciable variation in longitude for geometrical reasons. Professor Schlesinger's deductions were published in 1937, in the Monthly Notices of the Royal Astronomical Society,[12] and did indicate that a large part of the variations in longitude, that we had attributed to changing positions of the moon, could and should be accounted for on the basis of this annual term. Since that time, I have corrected all our previously published data by applying this annual term. Though not yet published, the resultant curve still persists in showing a maximum and minimum change in longitude across the Atlantic with the hour angle of the moon. This shows in the exchange of time signals between the Naval Observatory and both Greenwich and Paris, but not across the English Channel. The magnitude of the variation with the lunar day, however, is reduced in amplitude by the correction to about one-half the former value assigned by Mr. Loomis and myself. If the remaining variations, therefore, are translated into linear values, there remains a variation of about 32 feet to be accounted for in the distance across the Atlantic, that correlates with the position of the moon.

To add zest to the controversy, a publication was shortly forthcoming from the Jesuit Observatory at Zi-ka-wei, China, showing that a study of the intercomparison of time signals between Shanghai and the European stations at Nauen and Bordeaux revealed periodic variations in longitude between Berlin and Zi-ka-wei, dependent upon the moon's position. The explanation advanced was an earth tide causing a linear variation in distance of 60 feet between central Europe and the east coast of China. The amount of the variation given was several times larger than any probable error of the observations.

Unfortunately, after June 1934, shortly after the publication of our original papers, the daily intercomparisons of the 17.6 Kc. transatlantic time signals between our own Naval Observatory and the observatory at Greenwich ceased, due to changes in the transmitting station at Annapolis. We were obliged, therefore, to try to continue this study through time signals on short wave-length or high frequencies. The short wave transmission, however, across the Atlantic is so unreliable, particularly during the summer, that the project had to be abandoned until the 17.6 kilocycle frequency could be re-established. Through the coöperation of the American Geophysical Union, and Captain Hellweg, Superintendent of the Naval Observatory, the time signals were resumed on the 17.6 kilocycle frequency in 1939. However, hardly had the interchange of time signals on this reliable wavelength been re-established before the European War broke, and we have since been deprived again of this source of data.

Meanwhile, an exhaustive study was begun at the Naval Observatory, to see if any lunar effect in time observations could be detected from observations made at Washington with the photographic Zenith tube. The results of this series of investigations seemed to show that observations from March 1934 to February 1939 showed a small correlation with the moon of only such magnitude as to be consistent with an earth tide calculable on the basis of the accepted constants for the earth's rigidity. If apparent variations in longitude from time comparisons on two sides of the Atlantic were due to earth tide deformation, one would expect that results at either of the two observatories concerned would show a certain dependence of meridian observations upon the hour angle of the moon. However, it should be stated that, between the date of publication of the paper by Mr. Loomis and myself and the date at which the observations at the Naval Observatory were studied, our intercomparison of the time signals showed a decadence in the amplitude of the "lunar effect."

Since the complete abandonment of the 17.6 kilocycle transmission, no further study could be made. Meanwhile, other computers at the Naval Observatory have investigated latitude observations made at Washington and have found no variations with the moon, other than what could be accounted for on the accepted theoretical earth-tide basis. I venture, however, to continue to stress that, in any rigid harmonic analysis of a long series of observations, only those terms will persist that show a uniform rhythmic oscillation throughout the whole period of time in which the data were collected. Such a method of analysis does not allow for the possibility of an intermittent effect, such as might occur were deformation irregularly resisted, as might well occur in creep phenomena. The last chapter in the discussion of this problem does not yet appear to have been written.

In arriving at our earlier published conclusions, Mr. Loomis and I pointed out that, in interpreting the variation in time signals exchanged across the Atlantic, we had tactically to assume that the speed of the radio waves carrying the signal was the same in passing from west to east as in passing in the opposite direction, and that the velocity of the wave does not substantially change during the interval covering each day's exchange of signals,—an assumption which should be reasonable, but which can not indefinitely pass unchallenged. Experiments for the verification of such an assumption have already been planned[13] and should be carried out after the termination of the war.

May I now call your attention to such evidence as exists for two of the most astonishingly large changes in geographical coördinates that have ever been published from the study of the intercomparison of astronomical determinations? One of these concerns Greenland and the other South America.

In the summer of 1932, a Norwegian, Hans Jelstrup,[14] conducted an expedition to Sabine Island, in the Melville Bay region, off the west coast of Greenland. The purpose of his expedition was to reoccupy a station whose latitude and longitude had been determined by Börgen and Copeland in 1867 and 1870. Jelstrup identified the identical pier used by the observers in 1870. The results of the new determination gave the position of this pier as 615 meters west of the position given by Börgen and Copeland in the 1870 observations. The comparison of the longitude determined by Jelstrup with that given by Börgen and Copeland in 1870 has been questioned on the grounds of inaccuracy of the earlier observations; and Jelstrup's conclusion that, in 1932, the station was 615 meters west of the earlier position, has been warmly challenged.

It is true, that Börgen and Copeland did not have the advantage of the modern radio for time comparison, nor the use of a cable for receiving Greenwich time. They were therefore obliged to depend upon lunar culminations and observations of occultations for determining Greenwich time. These methods are well recognized as quite unsatisfactory and lacking in precision, compared with the telegraphic or radio method. It is of interest, however, to find just what degree of reliability could be placed upon those early determinations. For this reason, I have taken the trouble to examine with considerable care the original detailed publications of the 1870 expedition.[15] Their probable error was about plus or minus 1.1 seconds for either method. The longitude determinations from the four-star occultations agree within 1.5 seconds of the longitude found from the sixteen lunar transits. This is as good an agreement as could be expected in the case of the methods used.*

The uncertainty of Börgen and Copeland's observations of longitude, expressed linearly, amounts to about 80 meters, while the modern observations of Jelstrup in 1932 could represent an uncertainty of not much more than 20 meters. Now, if we allow for the worst possible combination of these estimated errors, it would appear that the results in 1932 could be expected to agree with the 1870 observations within about 100 or, at least, 200 meters, whereas the position actually found by Jelstrup was indicated to be 615 meters west of the position determined from the earlier measurements. The conclusion seems to be that, in the 62 years interval, Sabine Island had moved west by at least two or three hundred meters.

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* Observations were also made by Green in 1870 to determine corrections to the moon's position, thus minimizing uncertainty due to this possible source of error.

Critics of this interpretation should bear in mind that there is also evidence of movement from the comparison of the latitude determinations made in 1870 with those made in 1932. Jelstrup's value for the latitude was 3.2″ less than that found by Börgen and Copeland in 1870, an amount which indicates that the pier, as was occupied, had moved 100 meters south of the 1870 position. Whatever doubt may be cast upon the interpretation of the longitude differences, it should be emphasized that the latitude determinations were entirely independent of the exchange of time signals, and that the order of accuracy attained for latitude determinations in 1870 was probably nearly as good as in 1932. The uncertainty involved would appear to rest chiefly upon the question of the positions of the stars used. The conclusion of the Börgen-Copeland and Jelstrup expeditions would seem to indicate that Sabine Island had drifted south and west some seven hundred to eight hundred feet during the 60 years that had elapsed between the expeditions.

Now, for the other startling incident! In 1931, a redetermination of the longitude between the Argentine Observatory at Cordoba and the Royal Observatory at Greenwich was made, and the results compared with the position of the observatory made under the direction of Dr. Benjamin A. Gould in 1871.[16] Every astronomer recognized Benjamin Gould as one of the most exacting and painstaking of observers and mathematicians. It would appear that he would have used every precaution to secure highest attainable degree of precision possible in his time. Dr. Gould based his position upon two very careful longitude determinations carried out by the U. S. Naval astronomical expeditions. These expeditions derived, independently, the longitude of Buenos Aires from Greenwich and that of Valparaiso from Washington. One of these determinations was made by way of the Atlantic and one by way of the Pacific. The results of the two determinations showed a satisfactory agreement within 0.05 seconds, or a linear uncertainty of some 16 meters.

In the determination of 1931, time signals were exchanged by radio in place of cable and the probable error from 35 nights' observations gave an uncertainty of the resulting longitude of but 0.007 seconds. When, however, the 1931 result was compared with that of 1871, the two determinations differed by 1.11 seconds, corresponding to a linear distance of 400 meters or 1200 feet,—an unthinkable difference to be charged to the account of observational errors.

In discussing the results, astronomer Zimmer, of the Cordoba Observatory, tacitly assuming that the observatory could not have moved, commented on these results as follows: "The remarkable thing about it is that two independent determinations such as those made by Green and Davis, should be in complete accord, and yet be wrong by a whole second. We have to assume that those two determinations were in error by at least a round second." Mr. Zimmer found it impossible to imagine any continental drift to allow for the change that had taken place, especially since he stated that, if one were to imagine any such movement, "the direction was opposite to that predicted by Wegener." One, perhaps, should be cautious in evaluating such a statement, lest it lead to a type of reasoning that may discourage discovery, by inhibiting the imagination, on the one hand, and erroneously assuming, on the other hand, that if a lateral movement had taken place, it would have to be in accord with some particular hypothesis. Perhaps, it is a pernicious imagination that would suggest that, if an orogenic movement had at one time been westward, there might be a reactionary period when a crustal shift could, for the time being, take place in the opposite direction. This is something for geologists to ponder over.

In closing this discussion of longitude variations, there remains one other observation to mention; namely, the results of a comparison of longitude determinations between San Diego and Washington, made during the campaigns of 1926 and 1933, and published in 1939.[17] A comparison of the observations of longitude reduction over this seven-year interval, taken at face value, showed that the longitude determination in 1933 was 0 045 seconds greater in 1933 than in 1926, an amount more than six times the probable error of the observations. This amount expressed in linear dimensions would correspond to an increase of 55 feet in the distance between the east and west coast. This discrepancy seemed so much larger than errors in the observations could allow, that the Naval Observatory astronomers re-examined the reductions of the 1926 observations and came to the conclusion that a small correction to the reading of the chronograph records by the earlier computers could be made so as to bring the results of the two series more nearly in accord. Even after such corrections have been applied, it is found that, in 1933, San Diego appears 37 feet farther west than the 1926 observations indicated.

One should emphasize here that there has been a vast improvement in the technique of determining longitude in the last fifty years, largely through the advent of frequent time comparisons by radio. In place of being dependent upon cable lines, one can receive at will, in any field station, time signals broadcast with an accuracy undreamed of a century ago. Nevertheless, with every advance in obtaining another decimal place, one is sure to encounter new problems to be solved. In this case, we have no exception to the rule. In recent studies that we have made on the apparent elapsed time in the propagation of radio signals between distant points, it has been found that it is no longer safe to assume that the velocity of these radio waves is constant and equal to that of the velocity of light, an assumption all too easily made by those who have not faced directly some of the problems of communication by way of the ionosphere.

In a paper published in the Journal of Terrestrial Magnetism in 1936,[18] I showed that, in deriving the velocity of propagation of time signals employed in the longitude campaign of 1926, the apparent group velocity of the radio waves deduced on the assumption that the waves traveled the shortest possible path between stations varied in speed, all the way from values approximating the velocity of light to values approximating but a little more than two-thirds the velocity of light. Furthermore, there was indication that the speed appeared to depend more or less directly upon the intensity of the earth's magnetic field in the region over which the waves passed.

Even in the case of constant comparisons between two fixed stations, such as Greenwich and Washington, it was found that, over a period of six years, the velocity of the radio time signals could vary appreciably, for reasons not readily explained. The inference from this would seem to be that, in attempting to obtain a still higher degree of accuracy in determining longitude, we are involved in a study of ionospheric conditions, upon which the peculiarities of radio transmission must depend. For a higher degree of precision in longitude determinations, it would appear desirable that the observing stations involved should be in relatively close proximity to wireless stations which transmit the signals.

I now dismiss completely the matter of astronomical determinations and call your attention for a moment to the possibility of measuring short periodical displacements of the earth's crust in the vertical direction. Thanks to the improvement in gravimetrical instruments within the last few years, it has been possible to measure variations in gravity with a degree of precision never before attained. While the modern gravimeter has been developed ostensibly for commercial purposes, one of these instruments placed for several months in a fixed location has revealed so unmistakably the rise and fall of the earth's crust in response to solar and lunar tides in the earth itself as to render the results unquestionable.

A few years ago, I took the opportunity of visiting the Gulf Research Laboratory in Pittsburgh, where, under the direction of Doctors Foot and Eckhardt, a sensitive gravimeter, set up on test in the basement of the laboratory for several months, had shown consistently both the solar and lunar tides in the solid earth.[19] The tidal curve reduced from the gravimeter observations, when compared with the calculated theoretical tide potentials, showed a definite phase lag of about 50 minutes between the time of the maximum tidal force and the maximum response of the earth's crust. While the experts in charge of this instrument felt that caution should be exercised in determining the amplitude of the curve, because of certain uncertainties in the calibration of the instrument, one could state with certain reservations that the range of movement of the earth's crust in the vertical direction during the lunar day represented a displacement of about 2 feet. Were there only this single incident, it is quite sufficient to emphasize the importance of the establishment of a number of such gravimeters in selected regions, to determine not only such differential displacements, but also to discover, if possible, regions where such displacements may exceed or fall short of the magnitude of the variations recorded at Pittsburgh.

As a possible cosmic cause for differential movements in the earth's crust, one can not forget the gravitational force exerted by the moon. It is always difficult to see how the very feeble stresses set up in the earth's crust by the lunar tidal attraction could have an appreciable effect in moving large continental masses. However, perhaps one should not forget the possible resultant effect through the continued application of intermittent stresses repeatedly applied in a given direction. Were it possible to think of extended regions of mobile matter within the earth, then perhaps tides set up in this underlying mobile material could produce, through hydrostatic pressure, far-reaching effects in this connection.

A few years ago, I undertook a reinvestigation of the frequencies of the occurrence of major seismic disturbances, as possibly correlatable with the position of the moon.[20] While the results were not as conspicuously convincing as one might have wished, there was a definite indication that, at least, so far as deep-focus earthquakes are concerned, the curve of frequency deduced showed two maxima and two minima during the lunar day. Maximum values coincided with positions of the moon four hours and sixteen hours past the meridian. One might say that there are indications here of a trigger action, such that if, through accumulated stresses, large earthquakes are about to occur, there is a better chance of their occurrence during a time when the horizontal tidal component of the moon's gravitational force is at a maximum.

A few days ago, I received a communication from that venerable and esteemed geologist, Professor William Hobbs of Michigan, in which he said, "If you have published material proof of the changes of latitude and longitude, could you send reprints, or else references to place of publication?" By training and temperament, I think any scientist should be reluctant to use the word "proof," and, while the material I have presented may have fallen far short of what any mathematician could desire as proof, I hope I have fairly, and without undue prejudice, presented evidences that exist, in such a way as to encourage a degree of open-mindedness among both astronomers and geologists on so fundamental a question as that of differential movements now going on in the earth's crust. If so, then I shall have been successful, at least to the extent of having presented material consistent with the subject assigned.

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SECTION OF BIOLOGY

April 10, 1944

Professor W. M. Copenhaver, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *Effects of Liver Extirpation and Transplantation on Blood-Cell Formation.* (This lecture was illustrated by lantern slides.)

Since the liver is an organ with many functions, its removal produces a number of physiological disturbances. The present study has not attempted to cover all of the disturbances produced by the extirpation of this organ. The major emphasis has been given to the effects on the blood and blood-forming organs.

The present experiments on amphibian embryos were stimulated originally by the work on anemia in mammals, particularly by the work of Whipple and co-workers (1920, 1925) and Minot and Murphy (1926). Through their work and that of Castle and others, considerable information concerning the role of the liver in erythropoiesis has been gained. Castle has shown that the interaction of an extrinsic factor present in food and an intrinsic factor present in the normal gastric secretion leads to the production of an antipernicious anemia factor (erythrocyte maturation factor) which is stored in the liver, and, to some extent, in the kidneys. Pernicious anemia occurs when a lack of the gastric factor leads to a deficiency of the maturation factor. From the available evidence, we should probably expect an anemia after surgical removal of the liver, although, as pointed out by Castle (1936), the kidney is also a storehouse for the maturation factor.

A number of investigators have studied the effects of complete hepatectomy in mammals, but these studies have little bearing on the present problem because mammals usually do not survive the operation for more than 10 to 15 hours. This is too short a time for changes in the blood picture.

Adult amphibians survive hepatectomy longer than mammals, but it is doubtful whether they survive long enough to develop an anemia initiated primarily by the lack of an anti-anemic liver factor. The literature on this has been discussed more fully in an earlier report (Copenhaver, 1943).

Liver extirpation and transplantation in amphibian embryos was studied by Holtfreter (1925), but his chief purpose was an experimental study of the development of the organ. He made no mention of anemia, but, from the present experiments, it is evident he did not keep his liverless animals long enough to obtain any effects on blood formation.

Amphibian embryos are particularly useful for the problem because the technique of hepatectomy is relatively simple (as compared with mammals) and the embryos survive for a considerable period. On the other hand, embryos have certain disadvantages, particularly their small size, which makes it difficult to obtain enough blood for good smear preparations.

NORMAL BLOOD DEVELOPMENT IN *AMBLYSTOMA*

As in mammals, several locations (mesoderm of yolk region, liver and spleen) are utilized for erythropoiesis at successive ages. After the spleen differentiates, it becomes the chief location for erythrocyte formation for the remainder of the life of the salamander, while a layer of tissue beneath the capsule of the liver functions for the development of granular leucocytes. In this respect, the salamander differs from mammals, where the succession of developmental stages leads to the formation of erythrocytes and granular leucocytes in bone marrow.

The morphological stages of hemopoiesis in salamanders have been illustrated clearly by Jordan and Speidel (1930). As in mammals, the earliest cell which contains a recognizable amount of hemoglobin is called a proerythroblast, and successive stages with more hemoglobin are called erythroblasts and normoblasts. The mature erythrocyte is a nucleated, large, oval-shaped cell which differs considerably from the smaller, non-nucleated human red corpuscle. The mature erythrocyte of the salamander is larger than the immature stages which precede it; in mammals, the reverse is true.

EXPERIMENTAL METHODS

Two species of salamander were used, *Amblystoma punctatum* and *Amblystoma tigrinum*. The majority of the operations were done on embryos at Harrison's stage 39, a stage 2 to 3 days after the beginning of circulation and only a few days after the initial swimming reactions. At this stage, the liver is relatively undifferentiated and consists of a pouch of yolk-laden endoderm attached to the floor of the gut.

The experiments fall into two main groups. In one series, the liver anlage was removed and the embryos were reared as long as possible to study the effects of complete extirpation. In another series, the operation was done in two stages. First, a liver rudiment was transplanted from one embryo to the tail of another embryo and then, after a few days, the normal liver was removed from the host.

EFFECTS OF EMBRYONIC HEPATECTOMY

A total of 251 embryonic liver extirpations were done in this series, but some embryos failed to survive the immediate effects of operation, and others were fixed for histological study at different ages after operation. One hundred seventy-nine animals were kept until they died, before being fixed for histological study. Some of these were reared under normal conditions, others were fed on liver, and still others were given injections of liver extracts. The extracts were supplied by Dr. Randolph West and were known to be effective in the treatment of human pernicious anemia. There was no significant difference between the survival periods of the untreated animals and those given liver extracts. The maximum survival for untreated *Amblystoma tigrinum* was 41 days after operation (33 days after the yolk resorption stage), with a mean of 20.5.

All animals which survived much beyond the yolk resorption stage became very anemic. It is interesting that anemia never developed before yolk resorption, but feeding the animals on yolk of younger embryos did not prevent it. The anemia was observed first by studying the gill circulation of living animals and was checked later by serial sections. In extreme cases, there was complete absence of mature erythrocytes and only a few erythroblasts.

There was no liver parenchyma, but the ventral mesentery of the liver region contained developing leucocytes similar to normal subcapsular hemopoietic tissue. The differentiation of this tissue is apparently no dependent on the development of the liver.

The spleen was hypoplastic and its vessels were usually empty.

LIVER EXTIRPATION COMBINED WITH TRANSPLANTATION TO AN ABNORMAL POSITION

In this series, the animals had no liver tissue except the graft. Although they did not grow as rapidly as normal animals, they survived for a long period. Some were reared beyond metamorphosis. They showed no signs of anemia.

Sections through the transplants showed parenchyma cells arranged in cords between well-developed sinusoids. Bile canaliculi and bile ducts were present, but they did not connect with the gut.

The grafts had vascular connections with the caudal artery (dorsal aorta of tail) and with the caudal vein. Although the main blood supply was arterial, in contrast to the normal condition in which the major supply is from the portal vein, the parenchyma cells stored a considerable amount of glycogen.

In a number of cases, the grafted liver was removed after the animals had been kept for a period ranging from 65 to 132 days. The maximum post-operative survival was 8 days. These older animals died with symptoms of liver deficiency similar to those described by Magath and Mann (1925) for dehepatized frogs. They did not become anemic; the survival period was probably too short for changes in the blood picture.

DISCUSSION

In attempting to explain how embryonic liver extirpation causes anemia, a number of factors have to be considered. Whipple (1922) and Robscheit-Robbins and Whipple (1925) have concluded, from their studies of anemia in dogs, that liver produces a parent pigment complex which is essential for the formation of hemoglobin. Animals without a liver would be expected to lack the parent pigment substance necessary for hemoglobin formation and they would also be expected to have a deficient amount of the erythrocyte maturation factor. Lack of pigment should reduce the quantity of hemoglobin and the size of the erythrocytes, whereas the lack of the maturation factor should produce larger cells or macrocytes as in pernicious anemia. It should be possible to check these points, but in most of our experimental animals there were very few erythrocytes left when the animals were fixed for sectioning. In most cases, the animals passed from an approximately normal to an extremely anemic condition within a relatively short time. From a study of serial sections, the few erythrocytes present in the final stages did not appear abnormally large.

Another difference between the anemia produced by embryonic hepatectomy and human pernicious anemia is that, in the former, the blood-forming organ (the spleen) is very hypoplastic, whereas, in the latter, the blood-forming organ (the bone marrow) is hyperplastic with many immature cells.

It might be supposed that the experimental anemia is produced by removal of a blood-forming organ. The liver is normally an organ for erythrocyte formation for a period of embryonic development, but since the anemia does not become pronounced before the spleen normally begins to function in erythropoiesis, the results do not prove that the liver is necessary as a locus of blood formation. The decreased size of the spleen is probably not a primary cause, since Jordan and Speidel (1930) have shown that splenectomy in adult *Triturus* is followed by compensatory erythropoiesis in the general circulation. The lack of erythrocyte proliferation, either in the small spleen, or in the general circulation, appears to result from the absence of some substance normally produced in the liver.

The fact that all liver extract injections have thus far been ineffective indicates that the cause of the anemia is not simply the lack of storage of the erythrocyte maturation factor. Functioning liver cells, such as were present in heterotopic grafts, seem to be necessary.

In summary, it may be stated that amphibian embryos survive complete liver extirpation for a period which is sufficiently long to produce anemia. The anemia does not occur during the embryonic yolk stages, but begins about 1 week after yolk resorption. In the final stages, from 20 to 40 days after operation, there are few or no erythrocytes left in circulation. The anemia has not been altered by injections of liver extracts but can be prevented by transplantation of liver tissues to other parts of the body.

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SECTION OF PSYCHOLOGY

April 17, 1944

Doctor Eugenia Hanfmann, Assistant Professor of Psychology, Mount Holyoke College, South Hadley, Mass.: *Approaches to the Intellectual Aspects of Personality.*

The majority of psychologists, today, agree that personality should be conceived of as a patterned whole, all parts of which are interrelated. Yet, this recognition of the structural relatedness of all aspects of personality has influenced but little the psychologists' treatment of the intellectual functions of the individual. MacKinnon, in his recent survey of the theories of personality, describes the situation as follows: "Character and temperament have loomed large in all discussions of personality, but not intelligence. . . . It is a neglect which cannot be justified, for certainly intelligence is as integral a part of the personality as are character and temperament."

Whatever the reasons for this neglect of intellectual functions as a personality aspect, a re-orientation must take place in this field.' The problems that must be considered are not new to psychology as such. The existence of qualitatively different patterns of intellectual performance has been recognized and these patterns have been extensively investigated in the field closely relatd to the psychology of personality, namely, in developmental psychology. This term is used here in the wide meaning given to it by Heinz Werner, namely, as the study of development of mental life in an individual, or in a social unit, or in an animal species. Taken in this wide meaning, developmental psychology includes also the study of regressive phenomena found in the field of psychopathology. Since my own work on intellectual processes has been largely in the abnormal field, I shall briefly review some of the experimental and clinical studies carried out with the schizophrenic and organic cases, and then I shall try to show how similar methods of investigation could be applied to the problem of the personal patterns of intellectual performance in normal subjects.

If one is studying intellectual processes with the view of discovering qualitative differences rather than simply measuring achievement, one must present the subjects with tasks that permit a wide variety of solutions, or of methods of solution. One such task we found in the picture completion test devised by Healy. The subject is presented with a series of pictures from each of which a square piece has been cut out, and is asked to choose from a number of small picture blocks the one that best completes the sense of each picture. Upon giving this test to a group of patients of a state hospital and to a group of normal controls, we found that two-thirds of the patients with organic brain pathology and one-third of the schizophrenic patients showed incongruous or absurd placements which were practically absent in the normal group. These peculiar placements were of different types. Sometimes the patient completed only the represented action, without any regard for the rest of the situation. Thus, in the picture representing children in the fields who are looking up at something, the subject may place in the portion of the picture representing the sky, a man, an automobile, or a dog, commenting simply: "Those boys are looking up at a dog." Or he may go even farther in his disregard of the physical reality and not refer at all to the outward action represented in the picture, such as looking at, or pointing to an object. Instead, he bases his placement on psychological moments, such as the thoughts, needs and wishes of the person represented. The picture of a tie, for instance, is placed on the horizon with the comment: "The boy needs a tie for his collar," or a horse is in the room because "the boy wants a horse—he wants to become a traveller." Finally, even the specific connection of wishing or thinking, obtaining between the person and the object, may be dissolved in favor of a more indefinite impression that a certain object belongs within the sphere of the represented situation or activity. Thus, a milk bottle may be placed on the wall because "people are having breakfast."

Occasionally the patient's wishes or fears influence the completion. One patient, for instance, decides against using the block with the picture of spilt ink, because, as he says, he himself would not want to drop the ink. Or he may protest that he is not the one who smashed the face of the boy in the picture. The picture, in such cases, seems to be given the status of reality.

Completions colored with personal reference were more common in the schizophrenic than in the organic group. The same was true of another kind of peculiar completions, which might be described as symbolic summaries of the pictured situations. The object inserted does not belong to the same concrete sphere of action as other objects in the picture: rather, it summarizes the pictured scene, giving its meaning, or its keynote. Thus, pictures of books are used as providing a key to school scenes: "The book is up—that means the boy is going to school"; a broken flower pot is inserted into the picture representing an accident, because "the boy's head—his mind—is broken, as is the flower pot"; clocks are made to stand for the events that usually fall into a definite time of the day: "six thirty—time to go to supper."

All peculiar completions have, in common, the disregard of spatial relationships. The inserted objects are placed in positions that are impossible within the spatial system represented: the room, the street, the field. It is this disregard of the physical impossibility of certain positions that makes most of the completions described appear incongruous or absurd to a normal observer. The questioning of the patients brought out clearly that they were well able to grasp the represented environment; e.g., in the picture where the boys were made to look up at a clock, they were understood to be playing in the field, with no object to which a clock could be affixed. Yet, even though the patients, in response to the examiner's urging, might occasionally admit that the position is strange, they showed no concern about it, and no desire to change the placement. From the comments of the patients we were forced to conclude that they were not unable to appreciate the spatial details, but that they simply denied the inserted object a position in space and, in general, denied the realistic character of the pictorial representation which a normal person in this situation usually takes for granted. The spatial relationships do not constitute for the patients a stable frame of reference which for normal subjects determines and limits the possibilities for various positions and sizes of objects. This lack of a spatial frame of reference may be considered as an example of general ineffectiveness of all stable frames of reference in schizophrenic thinking.

A second approach to the study of the schizophrenic thinking was made with the help of a sorting test devised by Ach and modified by the Russian psychologist Vigotsky. The test material consists of 22 wooden blocks varying in color, shape, height and size. On the under side of each block a nonsense syllable is written. Regardless of color and shape, *lag* is written on all tall large figures, *bik* on all flat large figures, *mur* on the tall small ones, and *cev* on the flat small ones. The subject has the task to find and separate these four kinds of blocks. After each incorrect attempt at grouping, the examiner turns up one of the wrongly selected blocks and encourages the subject to continue trying until the required solution is found.

The test may be scored quantitatively, in terms of the amount of time and help required for a solution. Its main value, however, lies in the possibility of a very detailed qualitative analysis of the procedure of the subject, whose manipulations of the blocks reflect nearly every step in his reasoning.

Normal subjects of high educational level see the task clearly as one of classification and try out different possibilities in succession: color, shape, size, etc. The groups of blocks they form in this process represent classes, all members of which have one characteristic in common. An extreme deviation from this logical procedure is shown by those patients who form groups based on similarities between individual blocks. To a *small red* triangle the patient may add a *small red circle,* next to this is placed a *large* blue *circle,* next to this a *large* square, so that the resulting chain of blocks has no common denominator. In producing such and similar complexes of blocks the patients, instead of formulating and testing hypotheses, seem to be passively led by the material at hand. Even when, with the examiner's help, the correct grouping has been established, the patient is unable to see the principle on which it is based and to reconstruct the groups after they have been destroyed. In this, the patients differ from the normal subjects of lower educational level, who frequently start grouping blocks in a rather primitive fashion, but are always able in the end to understand the basis of the correct solution. From one-third to one-half of the schizophrenic patients show definite impairment of conceptual thinking on this test, and the patients with brain pathology perform, as a group, on a still lower level.

Let us consider now, in more detail, the performance of the normal subjects of high educational level. Although all of them performed on the highest conceptual level, they differed typically in their approach to the task. One group preferred to formulate their hypotheses in advance, paying minimum attention to the blocks in front of them and using them only for the testing of their hypotheses. Frequently, these subjects even refused to touch the blocks until the classification was quite clear in their minds. The other group of subjects was quite absorbed in the material, got ideas for grouping in the course of the process of grouping itself, and frequently achieved the correct classification before having clearly formulated the conceptual basis for it.

The first type of approach was more frequent among men, the second among women. Regardless of whether the conceptual or the perceptual approach predominated in a given case, some subjects showed a good integration of these two aspects of performance, whereas others consciously fought the intrusion of perceptual tendencies, thereby lowering considerably the efficiency of their performance. We may assume that these different patterns of intellectual performance bear a significant relationship to other personality aspects, a relationship which should be further investigated.

The relation of intellectual to other aspects of personality may be seen clearly in abnormal cases showing an extreme change in intellectual processes. One such case, soon to be published, was investigated by K. Goldstein, M. Rickers-Ovsiankina, and myself. The case was one of brain pathology produced by injury to the head. The disturbance of cognitive functions was extreme, showing itself even at the level of perception. The patient was unable to recognize isolated objects, regardless of the mode of their presentation. In his attempts at naming them he would be led by different clues in succession, but would never achieve correct recognition. Yet, if he could successfully perform an action with an object—e.g., write with a pencil, unlock a door with a key, throw a ball and see it bounce—he would recognize and name the object immediately and securely. Because of this mode of recognition, objects offered in a setting permitting action (e.g., key with lock) were recognized better than those presented in isolation, and pictured objects that could not reveal their nature through real action were recognized worst of all. Application of definite categories which insure structure and relative constancy in the case of normal perception was missing from the perception of the patient. An object for him was only an integral part of an action, and had little meaning outside of this context.

The use of language was another function that reflected the drastic changes in the patient's intellectual processes. He was able to understand and use language only in connection with actual concrete situations. When being told a simple story he would look around for people and objects mentioned in it, and, not finding them, was unable to apprehend the meaning of what he heard. The use of verbal symbols as a means of operating at will, with non-present or even non-existing things, was closed to the patient.

The memory function was similarly disturbed. A few minutes after some episode had taken place, the patient was completely unable to recall it, if the situation had changed in the meantime and he was engaged in a different activity. Isolated questions directed at him were never sufficient to produce recall. Only if some ,aspect in the present situation vividly reminded the patient of a former one, was he able to recall it. In this case he re-enacted the whole scene which, for the moment, seemed to become reality for him. Yet, even his successful recall differed from the normal one, in that he was unable to localize the past event in time and to relate it to other events of his life, past or present. The memory of the patient provides him only with a number of suddenly emerging concrete episodes, but not with a continuous picture of his past. His whole experience totally lacks time-perspective.

How do these deep-going changes of cognitive functions influence the patient's emotional, evaluative responses to his environment? First, the patient responds emotionally only to the situations he is able to grasp, which limits considerably the scope of his response. On the other hand, he responds most intensively to the events he can and does grasp: thus, he always praised excessively all the objects that functioned adequately or fulfilled some useful purpose. In social situations the patient responded strongly and adequately to friendly approaches, to offers of help or of presents. He could always be relied upon to give away any of his belongings in response to somebody praising them. Equally strong negative emotions were occasionally aroused by situations of strong frustration. At first glance, these emotional responses seem to be understandable and essentially normal. Actually, however, they show many unusual aspects. The very strength of emotions is due to the abnormal narrowing down of the mind to the one momentary impression. The same close dependence of the concrete situation results in another deviation from the norm, which has a far reaching significance for the patient's relations to other people. Since people do not exist for him when they are not present and active, he has no permanent attitudes toward them and responds only to their momentary actions. Consequently, he never initiates any social actions himself, and no permanent personal bonds are formed between him and other people. Even within his own family, the patient remains essentially a stranger.

This brief description of a case of an extreme change in cognitive functions illustrates how the loss of cognitive frames of reference leads to deep-going changes in the total behavior of the patient and in his emotional attitudes. The connection is such a close one, that it seems hardly possible to isolate the cognitive functions as such from the rest of personality. Goldstein, therefore, is reluctant to speak of the disturbance of the cognitive functions as such, and prefers to speak of the loss of categorical behavior, which implies a change in all fields of experience and performance. It is to be hoped that, by applying a similar type of analysis to the investigation of qualitative variations within the range of normal intellectual performances, we shall in time be able to see these variations as reflecting significant differences in the underlying personality patterns.

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Doctor Ernst Kris, New School for Social Research, New York, N. Y.: *Art and Regression.*[*]

In this paper, I deal with the art of the insane. Before entering into my subject, I should like to comment upon the terms used in the title: In referring to art, I do not refer to activities that are in the nature of art, such as drawings of children or of the unskilled, but a social phenomenon which presupposes a certain level of performance. The term regression stems from neurology and acquired psychological significance through Freud. He distinguished various types of regression. He speaks of (libidinal or) historical regression if an individual returns to an earlier stage of his own development; that is, if the child of four, faced with deprivations, turns temporarily to thumbsucking. Historical regression occurs as incident in the limits of normal development. No psychopathological phenomena are known without some kind of historical regression.[1] Freud speaks of Ego regression or (topological regression) in order to indicate that the Ego has "renounced" some of its functions.[2] The functions of the Ego during sleep are "reduced"; a lower level of control prevails than in the waking state. However, the Ego has not ceased to exercise its influence. The chaos of the dream is an example of regular, temporary and partial Ego regression. The secondary elaboration of the dream, the attempt to adapt chaos to an apparent order, is the best known vestige on continued control. In the context tonight, both temporary and permanent regression of Ego functions will be discussed. We shall ask ourselves what kind of "temporary" regression occurs wherever art emerges, and what happens to art if, in a state of "permanent" regression, in insanity, the artist continues to produce art.

I have chosen so pointed a formulation, not because it is one which throws light on the dark problem of what is beauty and why man enjoys it, but rather in order to investigate a specific problem. Psychology of art is a dormant science, if a science at all. It therefore seems permissible to apply an old device of research: The study of "Mental Pathology," of "The Experiments of Nature" (Dilthey, 1886), has led to an understanding of many psychological processes in the normal. We may try for once to see what we can learn for an understanding of art from the products of some individuals who, known as artists, continued to work in the darkness of psychoses. However, I should like first to dispel an apprehension: I do not aim at any general theory which would once more try to link all art to insanity, nor do I recommend the creative genius in general as a subject for pathological investigation. In limiting my discussion to that of Ego functions, or rather of one Ego function, I hope to throw some light on the relationship between art and insanity, which has proved so inspiring and misleading at the same time.

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[*] This address was delivered at the meeting of the Section of Psychology, March 20, 1944.

[1] I will not here discuss the problems of historical regression any further, as I do not propose to deal with it. For definitions, see Freud's "Meta-psychological supplement to the theory of dreams," [1916], Collected Papers, 4. For a discussion of historical regression, see K. Lewin, "Frustration and regression," University of Iowa Studies, 18, No. 1, 1941. For other attempts at definitions, see Sears, R., "Survey of objective studies of psychoanalytic concepts," Social Science Research Council, Bulletin, 51, 1943.

[2] Ego regression also implies a genetic concept, one referring to stages of Ego development. Ego, in Freud's definition, is not the "self," nor the "I," but a system of biological control functions.

The problem was formulated first by Plato, in "Phaedros" and "Ion." Art, he says, is produced in a state of creative madness, in a "divine release from the ordinary ways of man," a state of trance or ecstasy which in many ways is similar to "madness produced by human infirmity." In psychoanalytic terms, this might be formulated as follows: In creating art, the artist is in a state in which the control of the Ego is reduced. A regression takes place, but the regression is temporary only, and the Ego uses it for its own purposes. In certain psychotic states, especially in schizophrenia, the regression reduces the ability of the Ego more permanently and more completely; the loss of function is (in the ideal case) irreversible as long as the condition inheres. The Ego does not "use" regression by regaining control over it but (at least frequently) the Ego is overwhelmed by regression.[3]

The first part of this formulation refers to inspiration.[4] Not all artistic creation derives from inspiration; neither all kinds nor one kind wholly. But, wherever art reaches a certain level, inspiration is at work.[5]

The phenomenology of the process of inspiration, the experience of the artist during creation, is well known from many self-observations related through the ages. Wherever men live, some similar process occurs when they create; though what they produce is not always in the nature of art. All self-observations assert the absence of conscious volition: *It* works, and not the man. "I have two or three times in my life composed from the wish rather than from the impulse," says *Burns*, "but I have never succeeded." To *Wordsworth*, poetry is a "spontaneous overflow of powerful feelings." *E. A. Housman*, whom I quote because what he says tallies closely with impressions gained by psychoanalytic observation of several individuals active in the arts, has put it in these terms:

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[3] For a more detailed theoretical formulation of the relation of Ego control and regression, see the following papers: Kris, E., "Psychology of caricature," International Journal of Psychoanalysis, 17, 1936, part 3; "Ego development of the comic," *ibid.*, 19, 1937; and Hartmann, H., 'Psychoanalysis ar [illegible]

[4] Kris, [illegible]
Harding, [illegible]
spirational nucleus" of artistic creation.

[5] Thus may seem a transgression from psychology to normative thought, i.e , to "aesthetics" in the traditional sense. I am not so sure that it is a transgression, but if so, it is one I intend. I believe that the most sensible, and in fact most frequently applied, evaluation of art, by contemporaries of the artists or by posterity at subsequent periods of time, refers, consciously or unconsciously, less often to skill or perfection than to depth of meaning and expression, which inspiration alone seems to convey.

> "In short I think that the production of poetry, in its first stage, is less an active than a passive and involuntary process; and if I were obliged, not to define poetry, but to name the class of things to which it belongs, I should call it a secretion; whether a natural secretion, like turpentine in the fir, or a morbid secretion, like the pearl in the oyster. I think that my own case, though I may not deal with the material so cleverly as the oyster does, is the latter; because I have seldom written poetry unless I was rather out of health, and the experience, though pleasurable, was generally agitating and exhausting. If only that you may know what to avoid, I will give some account of the process.
>
> "Having drunk a pint of beer at luncheon—beer is a sedative to the brain, and my afternoons are the least intellectual portion of my life—I would go out for a walk of two or three hours. As I went along, thinking of nothing in particular, only looking at things around me and following the progress of the seasons, there would flow into my mind, with sudden and unaccountable emotion, sometimes a line or two of verse, sometimes a whole stanza at once, accompanied, not preceded, by a vague notion of the poem which they were destined to form part of. Then there would usually be a lull of an hour or so, then perhaps the spring would bubble up again. I say bubble up, because, so far as I could make out, the source of the suggestions thus proffered to the brain was an abyss which I have already had occasion to mention, the pit of the stomach. When I got home, I wrote them down, leaving gaps, and hoping that further inspiration might be forthcoming another day. Sometimes it was, if I took my walks in a receptive and expectant frame of mind; but sometimes the poem had to be taken in hand and completed by the brain, which was apt to be a matter of trouble and anxiety, involving trial and disappointment, and sometimes ending in failure."[6]

The passivity of the experience of inspiration may be rationalized in various ways: Not the artist creates, but a voice from above. He is not responsible as a person, but the genius to whom he submits is responsible. If we look at the process, as we sometimes are able to do, with the psychoanalytic microscope, we find that an impulse suggested by an instinctual demand or drive reaches, in this state, the preconscious level ("with unaccountable emotion") and is elaborated without the awareness of the creator. The form then appears ready and complete before his mind. In all creative thinking, similar processes can be observed. Neither in science nor in statesmanship is thinking entrusted to awareness only; preconscious elaboration is part of almost all thought. But in art it is different. There always exists an immediate link between preconscious thought and basic needs or repressed memories—the Id, in Freud's definition.[7] Were we to represent this process in a model, we should say that the psychic system is to some extent open to the unconscious; that an Id impulse is elaborated on a preconscious level and this elaboration is then sometimes consciously, and with pain and struggle, further refined.

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[6] Housman, A. E., "The name and nature of poetry," New York: The MacMillan Co.; Cambridge, England: At the University Press, 1936, pp. 47-49.

The experience of inspiration reaches from trance to slight drowsiness, or simple immersion in work. But, however intensive the preoccupation of the artist, it is, as a rule, limited to the process of creation. When the work is done, the painter looks at his canvas, the novelist rereads his script, the poet his poem, and the musician listens to the first accords. A shift has taken place—the creator has become his first audience. And now, in him as in every audience, the process of creation is repeated. As one is gradually drawn into the work of art, one relives on a different level what the artist has said. The way leads from the conscious to the preconscious, and only then is unconscious material set in motion.[8]

Every work of art establishes a link between the artist and a public. Art, in society, serves various purposes. It may be moralizing or it may be inciting. Hardly ever is it quite free of such a function; but, at no time, is great art only subservient. The artist may address one other fellow-man, or he may be conscious of striving for the admiration of many. He may, like artists in some primitive societies, address a community of fellow-experts, who, with some sign of deference, bow to his genius. Or he may address the unstratified audience of mass society. *But art, at all times, is communication from one to some others.*

In clinical observation of those who create, we find that the idea of a public is ever present. The shift in narcissistic cathexis, from the self to the product, accounts for the importance of this relationship. The work of art is to the artist frequently—and during creation always—part of the self; and when, after its completion, he looks upon it, himself part of the public, he still is closer to what he has produced than all of the others. Their approval of his work is, in fact, the essential approval of his own self.[9]

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[7] The work of art may give a modified expression to these Id impulses, or they may be perceived as dangers; the work of art then serves as a defense. Mostly, it serves both functions at the same time.

[8] Kris, E., "Approaches to art," in S. Lorand, "Psychoanalysis today." The International University Press, New York, 1944.

I shall here attempt to summarize these introductory reflections: Inspiration in artistic creation is a state in which the Ego has loosened its control; in which thoughts and images drive to consciousness and are formulated in shapes and signs "understandable" to others. Their understanding need not be correct. Art is as frequently and as regularly understood as misunderstood, and it is the secret of artistic formulation that it leaves the way open to a great many possible interpretations. The state of elation which art conveys to the public is lastly due to the fact that the play of emotions, the shift in cathexis between the psychic systems, repeats a process similar in nature to that which the artist experienced in creation. In "recreation," one might say, we repeat the process of creation in reverse.

I now turn to clinical examples. Most authors who have studied works of art produced by the insane, Prinzhorn in Germany, Bertschinger, and others, in Switzerland, Marie and Vinchon in France, and Nolan Lewis in the United States, have stressed the similarity between insane and primitive figurative expression.[10] This similarity which goes very far indeed, so that occasionally sculptures produced in institutions in urbanized areas closely resemble carvings from Oceania. Examples of this kind are highly impressive. The general explanation of these phenomena follows from some of Freud's basic assumptions which I formulate as follows: Differentiation of psychological structure is the work of the Ego; the Id is undifferentiated; it is not subject to cultural differences or, more correctly, it is subject to them to a much lesser degree. When, in regression, the Id comes to the fore, uniformity prevails.[11]

The problem I intend to discuss is less general. I shall deal with changes in the work of three artists during psychosis. I choose artists rather than laymen, since "the art of the unskilled" opens up problems of a specific kind.[12] I choose artists who suffered from paranoid schizophrenia, since, contrary to the opinion of Prinzhorn and others, I assume that the nature of the psychotic process is of considerable influence on the type of change in creative behavior.

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[9] This brief summary does not take into account various, not infrequent, deviations: indifference or abhorrence of the artist in front of his work is by no means rare. We have reason to believe that such attitudes are related to the unconscious determinants of production.

[10] Prinzhorn, H., "Bildnerei der Geisteskranken," Berlin, 1923; Bertschinger, H., "Illustrierte Halluzinationen," Jb. f. psychoanalytische und psychopathologische Forschung, 1911, 3, 69-100; Marie, A., "L'art et la folie" Rev. Sci. Paris, 1929, 67, 393-398, "L'expression artistique chez les alienes," Arch. Intern. neurol., 1931, 50, 211-212; Vinchon, J., "L'art et la folie," Paris, 1924; Lewis, Nolan, D. C., "Graphic art production in schizophrenia," Proc. Ass. Res. Nerv. Ment. Dis. for 1925, 1928, 4, 303-308.

[11] The archaism in Ego-regression of certain psychoses is, since Storch's investigations, a well-known phenomenon. Storch A., "The primitive archaic forms of inner experience and thought in schizophrenia" (German, 1923), Nervous and Mental Disease Monographs, No. 36.

In limiting the problem to one psychosis where the psychobiological process of regression is known from a wealth of clinical data, we hope to gain ground for a discussion of the transformation which Ego functions undergo during the process.

Our material does not lend itself to a study of the time perspective of the process. The urge to create sometimes arises in prodromal or early stages of schizophrenia. The urge then resembles, occasionally, that which we are wont to find with some adolescents. In several cases of poets it is possible to study the gradual transformation of style, as Jaspers did in the case of Hoelderlin[13]; or as it might well be done with the poetry of John Clare (1793–1864), whose unforgettable stanza, "I AM," reveals a striking analogy to Hoelderlin's work. In the poetry of both, the attempt to regain contact with the world and the self produces a gradual change in style, which leads Hoelderlin to new fervor in expression, and gives to the poetry of John Clare a greatness never accessible to the peasant poet who, in 1820, had gained recognition by his tame and polite versification. Problems of such gradual transformation must here be excluded, because the data available do not permit inferences of this kind.

The first example is chosen for the purpose of introduction. The Swedish painter, Ernst Josephson, had a psychotic break in 1898, at the age of 37. In his youth he had been the leader of the younger generation of Swedish artists. At about 30, he was recognized in Paris, where his paintings were shown in the "Salon." For 18 years, during insanity, he continued prolific activity as a draftsman. The conventionality of his earlier pictures disappeared. Whatever he drew in insanity seemed to be of greater boldness. It is as if he were no longer bound by the conventions of his era.[14]

On the other hand, we find that no clear insight can be gained into the meaning of his work. If we compare a nude, painted for the fashionable public of his time in Paris, with a drawing representing David and Goliath produced in insanity, the difference is obvious. In every detail, the working of the primary process stigmatizes the product of the insane. Goliath has three eyes, as have the giants of myths. A mask replaces, on the armor, the protrusion concealing the genitals. In another drawing, God, creating Adam, looks upon the world out of eyes formed by straight lines radiating from a center, with an expression more intense than anything that Josephson had previously produced. Out of God's mouth, breath is infused in straight lines to Adam's mouth. This contact from mouth to mouth has an almost sexual character. But then there are lines in these and in other drawings in which meaning is gone and stereotypic repetition is dominant.

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[12] I have outlined these problems on a previous occasion ("Bemerkungen zur Bildnerei der Geisteskranken," Imago, 1936, **22**, pp 339-370). Lately some of my hypotheses were confirmed by Anastasi, A. & J P Foley, "An analysis of spontaneous artistic production by the abnormal," Jour. of Genetic Psychology, 1943, **28**, pp 297-313.

[13] Jaspers, H , ' Hoelderlin und Van Gogh," Berlin, 1922

[14] Similar changes are not infrequent. See: Anastasi, A & J P. Foley, "A survey of the literature on artistic behavior in the abnormal," Psychological Monographs, **52**, No. 6, 1940, pp. 23-28, 36-39, where the relevant literature is surveyed.

Josephson's work had considerable influence upon a later time. The work of his insane period was rediscovered in the 1920's, and was then re-interpreted by "expressionists."[15] The detailed psychiatric data concerning Ernst Josephson have not yet been published, to my knowledge, and so this first case remains fragmentary.

The second case leads one step farther. There are more data available, though the artist, F. X. Messerschmitt (1736–1783), lived more than one hundred years before Josephson.[16] He was an Austrian baroque sculptor of renown. In his early days, in the 1760's and '70's, he enjoyed the admiration of the courts of Bavaria and Austria. Already, in the 1760's, he had several attacks of paranoia schizophrenia with persecutory ideas; and, in the 1770's, he finally had to withdraw from office. He had been a teacher of sculpture at the Imperial Academy of Fine Arts in Vienna, and his colleagues there had detected signs of insanity. The medical profession concurred with their judgment and petitions were sent to the court in order to guarantee a pension to a disabled man of great merit. In insanity, he withdrew to a little town some 50 miles outside the capital, into Hungary, and there, in 1781, he was visited by one Frederick Nicolai, a philosopher of the German Enlightenment and a bitter opponent of all Romantic thought; so bitter, indeed, that he violently attacked what seemed to him bombastic aspirations of Schiller and Goethe.

The enlightenment of the 1780's had, in various ways, opened up interest in psychiatric problems in Germany, and a magazine was published dealing largely with psychopathology.[17] Nicolai came from this circle. He interviewed the sculptor, Messerschmitt, for several hours, and in a book of travels—an "Inside Europe," as we might call it, of his day,—he reported the conversation. It gives us a clear picture of Messerschmitt's delusion.

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[15] Kris, E., "Bemerkungen zur Bildnerei der Geisteskranken, Imago," 1936, **22**, pp. 339-370 (with illustrations).

[16] Kris, E., "Die Charakterkoepfe des F. X. Messerschmitt, Versuch einer historischen und psychologischen Deutung," Jahrbuch der Kunsthistorischen Sammlungen in Wien, 1932, pp. 169-228; "Ein geisteskranker Bildhauer," Imago, **19**, 1933, pp. 384-411 (with illustrations).

Before I refer to it, I should like to discuss briefly the group of works with which Messerschmitt, a famous portraitist, was mainly concerned during his psychosis. It was of a scientific nature and dealt with a study of human physiognomy. He was working on a set of bronze busts which should represent the facial expression of man in various stages of emotion. This task was, at the time, entirely new. Artists who, in the great academies, had studied physiognomy before had chosen a different approach. For each of the emotions they had suggested one type of head and had endeavored to find what type,—girl or matron, adolescent or mature man—was best fitted to represent rage, adoration, joy, or pain. In the 1740's, however, in England, anatomists had started out to study the problem of how *one human face reacts to the change of emotions*, thus indicating an empirical problem. Messerschmitt followed this lead, and, in the first bust of his series, he represented one face, sleeping, yawning, and smiling. The face was always his own. While he was developing more of his physiognomic studies, his delusion got hold of him. He suddenly was convinced that he had discovered secrets of nature, long hidden by the Gods; therefore, evil demons were persecuting him. The persecution was both of an anal and an oral nature; thus, the mouths of all his busts (there are more than forty of them) are closed, so that nothing can enter. He rationalized the closing of the mouth in saying that the red of the lips was an invitation to sexual excitement. It was a mistake of nature that men should have lips, since animals had none.

This jump is characteristic of schizophrenic thought. The relationship between the human and the animal head had been a much discussed topic among artists, and a long heritage going back to Aristotelian tradition was kept alive in the academies of art. Messerschmitt knew treatises on physiognomics which were based on comparisons between human and animal features. The defense against the intruder through the mouth, suggested by his delusion and this literary tradition, were condensed as ideas are regularly condensed by the primary process. Messerschmitt varied the rendering of the lips. In order to close them he sometimes put a band over them. At other times, as if in spite, the lip is made to protrude; and, in two cases, in heads which he refused to look at when he was visited by his interviewer, the lips are pulled into long, extended shape, which, as Messerschitt said, was meant to frighten the spirits. Here, clearly, the oral intrusion of the persecutor is depicted, and, we shall add, depicted with some greatness (Fellatio phantasy).

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[17] "Magazin fuer Erfahrungsseelenkunde als ein Lesebuch fuer Gelehrte und Ungelehrte." 1780 ff.

Before I comment further upon this case, I shall add what I know of the reaction of the public to the insane artist. Famous as he was, he could not be ignored, and shortly after his death, the rewriting of history began: he had never been insane, but a genius; he had left his teaching position, not because he could no longer teach and was a danger to his pupils, as the well-meaning professors had said, but, said the historians, because the professors were jealous of his greatness. If one takes the trouble to unravel the vicissitudes of local historiography, one finds that by the middle of the 19th century, some seventy years after his death, the understanding of Messerschmitt's colleagues and benefactors had been obliterated and, instead, the paranoid interpretation of the insane had been accepted as historical truth. He was set up as a genius unrecognized by his contemporaries. Needless to say, there is no foundation for this myth. It took me, in more leisurely times, several years to piece together the evidence concerning these distortions and the reaction of the public to the busts themselves. Over and over, it was attempted to interpret their meanings in terms of physiognomy: here was a laughing man, here a man in defecation. Such interpretations sometimes hold good. There is something of this or that expression to be found. But few people ever had the opportunity of looking at the whole series of busts. In order to ascertain the reaction to the whole series I conducted several experiments. If faced with all of the busts, subjects would continue to give names, i.e., "interpret" two, three, four, or five—up to eight. After that, they would lose interest, recognize something stereotypic, and end up with the discovery that, in spite of changes of hairdress and of shape, it was always the same head and always the same type of expression.

Let me summarize: Here is an artist whose insanity breaks into a clearly outlined program of work. In insanity, his work suddenly serves two new purposes: the first is linked to the original goal, but while he sets out to study the human face in its changing expressions, he ends up by standing before a mirror and changing his hairdress and his features in search of a natural expression. The clinic of schizophrenia knows similar phenomena. I am inclined to consider this as a process of restitution. In search for what no longer can be synthetized, substitutes are adopted—substitute expressions instead of real expressions, or grimaces instead of natural physiognomies.

The second purpose which the work is made to serve is even more dramatic. The search for natural facial expression is combined with a quest for a symbolism closely related to the content of the delusion. The mouth is either tightly closed or the lips are protruding; there is defense against intrusion of the demons, submission to their will or aggression against them. ("The heads with the protruding lips," the artist said, "should frighten the demon.") I shall be brief here in saying that this second purpose leads us to a function of human expression which is more archaic than art. In the schizophrenic regression, we see the emergence of apotropous masks, which are meant to turn away evil. What has started out as a study in human communication, ends up as a magic performance in which residues of intelligible expression and pure magic are intertwined.

In spite of their delusionary meaning, the busts never lost a certain attraction for the public. In one respect they met with the public's demands. They were produced in a period of transition from the realistic but unruly portraits of the earlier 18th century to the portraits of the 1780's in Greek and Roman style. Even in his psychosis Messerschmitt had not lost contact with the artistic currents of his time. In this respect, his work remained fully integrated. This indicates the limitations of Ego regression in his case.

The third case leads to another extreme. It was studied by Dr. Elsa Pappenheim and myself, in 1938, at the Psychiatric Clinic in Vienna, and will be published in full, shortly.[18] A man in his forties, by profession an architect, a sturdy fellow, had repeatedly been in insane asylums. The diagnosis was paranoid schizophrenia. The patient felt himself to be, at various times, God, and the son of God. His former relationship to a woman from whom he had separated played a part. She was at once Eva and the Virgin Mary.

The delusion was charged with political symbols: the insane was a Nazi revolutionary against the Catholics who were at the time dominant in Austria. An accidental circumstance facilitated the investigation. I had met the architect previously, and he remembered me. Since he knew of my interest in criticism of art, he was more willing than he otherwise might have been to discuss what he had recently produced, and what he produced in insanity.

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[18] For a summary see Kris, E., "Probleme der Aesthetik," Internationale Zeitschrift fuer Psychoanalyse und Imago," 1941/2.

From the study of his work we soon derived the insight that what he drew was not essentially a representation, not mainly an image to be looked at by an audience. The drawings had a different purpose. All of them were signed by the artist and carefully dated. The signature and many other symbols which he used indicated clearly that he who had produced the drawings was God himself, God the artist. However, the signature again was not meant to identify the creator of the work, but rather to make the work effective; therefore, if the effect of the work had to be reaffirmed, the artist used to sign his name to the drawing not once but repeatedly; and, occasionally, when we submitted to him drawings which he had made several years before, he would resign them in order to revalidate them.

The drawings were verdicts. If he drew the tombs of his enemy, the enemy was condemned and killed. If he drew the City of Light, surrounded by angels, it meant the decision that Vienna would become a City of Light, the New Jerusalem, like Berlin, enlightened by the Nazis, the new Rome. One might say that, in these drawings, the artist intended more to reform the world than to depict it.

One of the main symbols he used was a triangle with his—God's—initials written in it. One day, when an attendant had refused him a second cup of coffee, we ourselves watched the use of this symbol in pure magic. The artist showed us a paper napkin on which there was the triangle filled in pencil. We found that he had written down the name of the attendant previously. Then he had heavily crossed out the name. The attendant, he said, was condemned.

The case requires little elaboration. The regression to the phase of omnipotence of thought so frequently found in schizophrenia extends here to the use of symbols. The representation of these symbols becomes magic ritual. What we have seen in the case of Messerschmitt as one tendency which only partly disrupted his work, here becomes the purpose of creation.

Any theory regarding the relation of art to magic, which these case histories might suggest, requires further confirmation. I shall briefly discuss three areas where I hope that further research will produce relevant data and where some such data has already been obtained. First: Amongst psychotics in asylums there are some who, at the onset of psychosis, suddenly turn to creation. They fill pages with manuscripts or with drawings. No statistical evidence is known to me as to the number of these cases, nor is it ascertained whether all or most of them are schizophrenic. A not-too-cautious estimate published in Germany about twenty years ago mentions about 2% among all the psychotic inhabitants of asylums as subject to "spells of production." I am inclined to think that this estimate is much too high. The relationship between creation and psychosis is sometimes dramatic. Let me quote, as an instance, a case observed in 1932, in Spain. An illiterate agricultural laborer is brought to the asylum and he starts to carve with a piece of glass from the window pane, on the window sill. The doctors supply him with knife and wood and he carves on. After several weeks, when some of his symptoms disappeared, he was turned to garden work as occupational therapy. He exchanged the knife for the spade. The psychiatrists did not carefully watch the transition. After a few weeks a complete remission took place. Before discharge from the asylum, the patient was asked to add to the collection of his works one produced after recovery. He did not recall ever having handled a knife or ever having projected a vision into wood. Urged to do a favor for those who had cured him, all he produced was a barren jug, painfully cut out of a block of timber; whereas in his insanity, he had produced a set of fantastic and highly "symbolic" sculptures.[19]

There are many similar cases described in literature. From experience with some of them I am inclined to believe that the overflowing urge which drives to production is frequently connected with the magic meaning of the product. The insane creates, since the process of production is likely to change, in a magic sense, the world which he depicts or describes. Clearly, this is not a total but only partly an explanation of the phenomenon.

I speak with greater assuredness about the second area of research. It concerns unconscious determination of creation in painting and sculpture. In psychoanalytic observation of several individuals proficient in the arts, I have gained the impression that some of the elements which the psychotic process reveals are, as unconscious determinants, influential in all creation.

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[19] Villamil, "Matis intenso de religiosidad inconsciente del psiquismo humano," Las progressos de la clinica Madrid, 1933, No. 254; Kris, E., "Bildnerei," etc., *loc. cit.*, p. 343.

Wherever, in psychoanalysis, we observe artists, painters or sculptors, we find that, during creation, a complex process takes place. No model is simply copied. An image is absorbed by the eye and then reprojected. Creation involves destruction; every stroke simplifies something; but every stroke also is part of a new shape and therefore restitutes and constructs. The degree, to which one, the other, or both of these meanings, destruction and reconstruction, predominate, seems to vary. I do not believe that they are ever totally absent. In destroying and rebuilding the world, the full experience of control arises. Control means a supremacy of a God-like nature.[20]

In order to avoid the suspicion of having projected a theory into case histories, I quote from a case history published independently of my observations. A patient of Nunberg's describes the experience while drawing a model, in these terms: "When I draw it I have a feeling of tactile communication, as if every line and stroke palpitated under my hand, and through this process only I learn to understand the essence of the model, in taking it into myself . . . Then it is mine . . . If I want to redraw it, I am independent of the object, since I can reproduce it as frequently as I want; it comes out of myself."[21]

I have, with at least one painter, seen this process in a genetic perspective. As a child of three she started to draw in order to replace a flirtatious mother whom she frequently missed. As a woman of thirty, when a man unworthy of her love was successfully making claims to her emotions, she could free herself from the threat of masochistic submission by painting his portrait. The mechanism she employed in the solution of libidinal conflict is inherent in the process of artistic creation. While we cannot generalize as to details, these and similar observations seem to indicate that magic control through artistic creation is one of the frequent unconscious determinants of production in the arts. In mythology and the folklore of aesthetics, in tales about art and artists, which can be traced back for more than two and a half thousand years, in Western European civilization, for several millenia in Chinese civilization, art and artists are presented as magicians, as rivals of the Gods; and Prometheus, who is nailed to the rock because he competed with the sculptor God in making humans out of clay, is, I believe, the arch type of all artists. They, like him, pay with the pains of creation.[22]

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[20] For the relation to destruction and reconstruction see Sharpe, E., "Similar and divergent unconscious determinants underlying sublimation in pure art and pure science," International Journal of Psychoanalysis, **16**, 1935, p. 186 ff. See also, Levey, H., "A theory concerning free creation in the inventive arts," Psychiatry, **111**, 1940, pp. 229 ff.

[21] Nunberg, Allgemeine Neurosenlehre, 1932, p. 132.

I hope that clinical study in institutions and psychoanalytic observations of artists will make it possible more closely to elaborate on the relation of art and magic. But no stringent confirmation may be expected in the third area, in the history of the arts themselves.

My hypothesis is that one of the roots of art—if we may speak of roots in a concrete sense—leads directly to magical ceremonies. Similar hypotheses have been discussed by various authors. I, myself, have tried to show that at least one branch of art permits us to follow the development from magic to art in concrete detail. I refer to caricatures. Three phases can be distinguished: There is first the magic level, found widely spread over the globe. Some idol representing a person is killed by some manipulation, by putting pins into it. Second: An image of the person is depicted, but the person is depicted as being killed. Capital punishment was painted instead of performed. This usage was current in certain civilizations, if an evil-doer had escaped justice. Third: The evil-doer is abused in image. His face is shown to be similar to that of the devil, and people are supposed to understand that, since he resembles the devil, he is the devil. This third stage is the decisive one, because here communication is made to an audience, which is given something to understand. The artist, instead of magically influencing the work, by executing punishment through sorcery, addresses the community in order to influence their judgment of the portrayed individual.[23]

I here summarize: Regression leads from communication to magic. Progression, according to this hypothesis, leads from magic to communication. Scientifically accessible is the former; and a psychology of art which we may one day obtain, complex as it will be, and based on the cooperation of many schools of psychological thought and on many data to be interrelated. Such a psychology of art might then sharpen our eye for an understanding of art as a phenomenon in the social history of man.

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[22] Kris, E , "Probleme der Aesthetik," *loc cit* , Zur Psychologie der aelteren Biographik, Imago, **21**, 1935, pp. 320 ff.

[23] See Gombrich, E., & Kris, E , "Principles of caricature," British Journal of Medical Psychology, **17**, 1938, pp. 319 ff , where the problem is stated in the following terms

"In the early primitive stage, the hostile action is carried out on the person through the picture. Every injury done to the picture is thought to touch the person and in reality wound the individual. Picture and person are one, damage done to the picture is damage done to the person. This is the stage of all practised effigy magic; which, of course, no longer exists in this ideal form in our culture

"In a second stage the hostile action is carried out on the picture instead of on the person, or else the picture is intended to perpetuate in graphic form a hostile action, injury, degradation or shame It is carried out only on the picture and does not wound the person himself, but only his honour. That is the stage of the defamatory, of shame pictures.

"In the third stage, to which caricature belongs, the hostile action is carried out by altering the portrait only. It remains in the picture, in the aesthetic sphere. This sphere denotes the methods peculiar to caricature. Thanks to the power of the artist the picture is altered through the medium of his art. Man is interpreted in a picture and only this interpretation contains criticism and aggression " (P 340)

SECTION OF ANTHROPOLOGY

April 24, 1944

Doctor E. Franklin Frazier, Howard College, Washington, D. C.: *A Comparison of Negro-white Relations in Brazil and in the United States.*[*]

Soon after the turn of the present century, Viscount Bryce expressed the opinion that the close and widespread contact of the advanced and backward races was the completion of a world-process which marked a crisis in the history of the world.[1] The process of which Viscount Bryce spoke had its origin in the economic expansion of western Europe during the fifteenth and sixteenth centuries, which resulted, not only in the spread of European culture, but in the expansion of the white race. Until the fifteenth century, the white race had been confined to western Europe and, only toward the close of the century, was the Moor expelled from the Iberian peninsula. In eastern Europe, the Asiatic still threatened the frontier. But in the fifteenth and sixteenth centuries, the maritime nations of Europe, in their quest for spices and gold, circumnavigated the globe and charted the course of their future colonies. Yet, it was to require two centuries before the colonizing white nations would secure settlements of importance in the newly discovered lands. The discovery and settlement of Brazil by the Portuguese in the sixteenth century, the settlement of North America by the Spanish and French in the sixteenth century, and by the English in the seventeenth century, were phases of the expansion of European peoples.

Although the initial impulse behind the economic expansion of Europe was the search for precious metals, the colonial powers soon began to exploit the productive powers of their colonies. In the tropical regions, the plantation system of agriculture became the characteristic form of industrial organization. In regions of "open resources," the plantation type of exploitation has required some form of forced labor.[2] In Brazil, as in the southern part of the United States, when the Indian proved inadequate, the African slave trade provided the necessary forced labor. Consequently, in both countries, the question of race relations is concerned primarily with the relations of whites and Negroes.

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[*] The materials upon which this paper is based were collected during a study of the Negro family in Brasil, made possible through a fellowship grant from The John Simon Guggenheim Foundation for the year 1940-41.

[1] James Bryce, "The relations of the advanced and the backward races of mankind." (Oxford, 1902), pp. 6-7.

In order to present an adequate analysis of the differences in race relations in Brazil and the United States, it is necessary to give at least a brief account of the conditions under which the Negroes were introduced into the two countries, their treatment under the plantation system, their emancipation, and their subsequent position in the economy of the two countries. Since Brazil did not offer the ready booty in gold, spices, silks, and precious stones as did the Indies, the Portuguese neglected the country for a quarter of a century. Then, during another quarter of a century, there were explorations along the coast and the country was divided into capitancies, which were held as feudal fiefs of the king of Portugal. During this period, the wifeless Portuguese adventurers began to mix their blood with the Indians and the offspring of such unions provided wives for later settlers. By the middle of the sixteenth century, Thomé de Souza, Captain-General of the colony, had founded the city of Bahia, or Bay of All Saints, which became the center of the African slave trade. Under Souza's administration, the cultivation and manufacture of sugar was begun in the settlements about Bahia. And it was to meet the demand for laborers that Negro slaves were introduced into the colony.

The southern part of Brazil did not develop as rapidly as the northern part during the early years of settlement. Owing to the failure of the cultivation of sugar, only a few Negro slaves were introduced. The colonists developed a sustenance economy, raising grapes for wine, wheat, and tobacco, and carried on a trade in Indian slaves. These settlers mixed freely with the Indians and there grew up in southern Brazil a population that was largely mixed with Indian. By the close of the seventeenth century, it is estimated there were not more than 100,000 people in Brazil who spoke Portuguese and, in this total, the pure whites were a minority.[3] The Portuguese officials constituted an upper caste, ranking higher than the Portuguese born in the colony. Next in rank came the Indian-white mixtures and, just below them, the mulattoes. After the mulattoes came various crossings of mulatto-Indian and Negro-Indian mixtures. At the bottom of the social pyramid were the slaves, the red slaves ranking above the black slaves.[4]

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[2] H. J. Nieboer, "Slavery as an industrial system" (The Hague, 1910), p. 385. "Among the people of the first category (open resources) the means of subsistence are open to all; everyone who is able-bodied and not defective in mind can provide for himself independently of any capitalist or landlord."

[3] Pandiá Calogeras, "Formação Historica do Brasil" 3a, Rio de Janeiro, 1938, pp. 32-33.

Beginning in the middle of the sixteenth century, the Jesuits carried on their missionary work for over a hundred years among the Indians. It was in the mission that the Indian submitted to the discipline of regular labor and ceased his resistance to the Portuguese. In 1570, the Jesuits secured a royal decree abolishing Indian slavery in the Bahia area and, in 1640, this decree was extended to all of Brazil. It has been stated that Negro slavery was introduced into Brazil at the suggestion of the Jesuits in order to save the Indian from annihilation. But, as Gilberto Freyre has pointed out, the Indian was not replaced by the Negro because of moral reasons or because of the Indian's pride and courage, as Indianphiles have said.[5] The Indian was not only physically inferior to the Negro, but he was a hunter and a nomad and in a more primitive stage of culture than the Negro. The Negroes imported into Brazil came from areas in Africa where agriculture and the arts had reached a high degree of development. Therefore, the Negro, because of his cultural heritage, provided the basis of Brazilian civilization in the north.

The number of Negroes imported into Brazil and their distribution in the country were governed by labor demands and the development of agriculture and industry in various areas. The cultivation and manufacture of sugar in the north drew the largest number of slaves. According to conservative estimates, 350,000 slaves were imported in the seventeenth and 1,000,000 slaves in the eighteenth and nineteenth centuries, for the growing of the cane and the manufacture of sugar.[6] Gold was discovered in Minas Geraës toward the close of the seventeenth century and reached its maximum production in the middle of the eighteenth century. For the mining of gold, 600,000 Negro slaves were imported. After coffee production began in 1820, a quarter of a million more slaves were imported. In addition to these importations, a million more slaves were imported for the mining of diamonds, the production of tobacco, cotton, food stuffs, and for domestic and commercial services. Altogether, according to a conservative estimate, three and a third million Negroes were imported into Brazil.

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[4] *Ibid.*, p. 33.

[5] Quoted in Roberto C. Simonsen, "Historia Economica do Brasil, 1500-1820," Rio de Janeiro, 1037, 1, pp. 199-200.

[6] *Ibid.*, pp. 201-205

The type of rural civilization which grew up in Brazil on the basis of African slavery has been described by Gilberto Freyre in his celebrated work, "*Casa Grande e Senzala.*"[7] As indicated in the subtitle of this book, slavery became the basis of a patriarchal economy. Under the patriarchal economy, the Portuguese masters and the Negro slaves lived in a type of close and intimate association that excells the most sentimental and romantic accounts of the social solidarity existing between master and slaves in the southern States. The close relationship between the two races in Brazil has been attributed by Gilberto Freyre to the fact that the Portuguese had occupied a marginal position, racially and culturally, between Europe and Africa. Concerning the widespread racial mixture which took place in Brazil, he writes:

> "One should remember that probably, owing to their intimate contact with the Moors, the Portuguese were from the beginning admirers of dark women. Girls, some of them blonde, were sent to Brazil from Portugal—sometimes by the Queen of Portugal herself—to marry Portuguese gentlemen established there as planters. These girls were sent to Brazil, not so much for the sake of race purity, as to preserve the social rank and the aristocratic status of the planters. But personally many of those gentlemen preferred Indian and even Negro and mulatto girls. . . . Some planters were careful in their importation of slaves from Africa to arrange that pretty girls should come. These sexual preoccupations were unworthy of Christian monogamic gentlemen; they show how much the Portuguese had come under the influence of the polygamous Arabs and the Moors. But one should not forget that those same sexual preoccupations led to the formation of a mixed race, as we have seen. Slaves from African groups far advanced in their culture, like the Mohammedan Negroes who knew how to read and write in Arabic, were imported to Brazil. And sexual selection had something to do with the importation of Africans from so fine a stock."[8]

The mixed-blood offspring of the sexual association between the two races enjoyed special privileges and were destined to play an important rôle in the history of Brazil. As the rural patriarchal organization disintegrated and urban communities began to dominate the political as well as the intellectual life of the country, the mixed-blood found an opportunity to compete on almost equal terms with the pure-blooded Portuguese. In a book describing the process by which the urban communities overcame the power of the rural patriarchal organization, Gilberto Freyre has devoted a chapter to the rise of the bachelor of arts and the mulatto.[9] These two elements in Brazilian society were responsible for its political and economic transformation during the nineteenth century.

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[7] Gilberto Freyre, "Casa Grande e Senzala," Rio de Janeiro, 1939. *Casa Grande e Senzala* may be translated as *the "big house" and the slave quarters.*

[8] Gilberto Freyre, "Some Aspects of the Social Development of Portuguese America" in Charles C. Griffin, Editor. Concerning Latin American culture (New York, 1940), pp. 83-84.

The abolition of slavery in Brazil did not occur until 1888, or nearly a quarter of a century after the Civil War in United States. Although, as Ramos points out, abolition came as "the result of a long process of evolution of public opinion,"[10] it was accompanied by heated controversies involving economic interests.[11] However, it was not accompanied by a violent civil war, as in the United States. There was no sharp boundary between free and slave territory, nor was there a well-defined conflict between an industrial and an agrarian economy. All classes were found in the abolition movement, some of the leaders being Negroes and persons of Negro descent. This was an indication of the extent to which Negroes and persons of Negro descent had already been assimilated into Brazilian society. One of the main consequences of abolition for the problem which we are considering is that it increased the mobility of the Negro population and facilitated the process of race mixture.

This brief account of the historical background of race relations in Brazil is sufficient to make apparent a number of important contrasts with the situation in the United States.

In a certain sense, one may not properly speak of race relations in Brazil. For, as Dr. Park has pointed out in an incisive essay, *The Nature of Race Relations,* "Race Relations . . . are not so much the relations that exist between individuals of different races as between individuals conscious of these differences."[12] In Brazil, there is lacking, both on the part of the Portuguese and the "white" Brazilians and, on the part of the "black" or "colored" Brazilians, a consciousness of racial differences. In fact, it is impossible to secure accurate figures on the various racial elements in the Brazilian population. In 1830, it was estimated that about 71 per cent of the population was white and of mixed blood and 29 per cent Negroes.[13] Since the emancipation of slavery in 1888, there has been an unwillingness to separate the population on the basis of race. However, an examination of 30,000 soldiers by an army officer gave the following percentages: whites, 59; mulattoes, 30; Negroes, 10; and caboclos (Indian-white mixtures) 1 per cent. These percentages are different from those obtained by Professor Roquette Pinto who, after researches in the National Museum, arrived at the following percentages for the various ethnic elements: whites, 51; mulattoes, 22; caboclos, 11; Negroes, 11; and Indians, 2 per cent. During his recent study of the Negro in Brazil, Pierson undertook to secure statistical data on race mixture in the city of Bahia.[14] He made an examination of the photographs of the first five hundred persons listed in 1937 as "brancos" or whites in the files of the Gabinete de Identificação.[15] He identified 68 per cent as whites, 19 per cent as mulattoes, 7.8 per cent as of white and Indian ancestry, and the remaining 5.2 per cent of Indian-white-Negro blood. In 1936, he undertook to determine by inspection the racial origin of 5000 participants in a festival in Bahia.[16] He classified 31.7 per cent as European, 18 per cent as Negro, 49.9 per cent as mulatto, and less than one per cent as of Negro-Indian ancestry. The figures are practically the same as those which were furnished me by Dr. Adolfo R. Leite, the statistician in the Department of Health of Bahia, who made his estimates on the basis of the school population and birth and death rates.[17] For the year 1938, he estimated the racial composition of Bahia to be as follows: whites, 32 per cent; colored, 49 per cent; and Negro, 19 per cent. As we shall see, the racial classification has little significance from the standpoint of race relations as we conceive them in the United States. In fact, the figures just cited are not so much an index to the distribution of races in Brazil as an indication of the extent to which a new race is being formed.

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[9] Gilberto Freyre, "Sobrados e Mucambos," São Paulo, 1936. The title of this book may be translated as *Two-storied town houses and huts.*

[10] Ramos, *op. cit.*, p. 54.

[11] João Dornas Filho, *"A Escravidão no Brasil"* (Rio de Janeiro, 1939), pp. 133 ff.

[12] Robert E. Park, "The Nature of Race Relations," in Edgar T. Thompson, Editor. "Race relations and the race problem," Durham, N. C.: Duke University Press, 1939, p. 3

[13] Arthur Ramos, 'The Negro in Brasil" (Washington, D. C., 1939), pp. 9-10.

In contrasting the racial situation in Brazil with that in the United States, much emphasis has been placed upon the difference in attitudes. This factor was of considerable importance in the early history of Brazil when compared with the traditional racial feeling in the United States. For, even during the early years of the seventeenth century, when Negroes had the same status as white indentured servants, distinctions based upon consciousness of racial differences existed.[18] As the importations of Negroes increased, this racial consciousness increased. This was not only true of the South where in many areas the Negroes outnumbered the whites and there were fears of insurrections. In fact, the intense racial feeling in the South has sometimes been attributed to fear of insurrections. However, in Brazil there were numerous slave insurrections which were attended by greater success than those in the United States. Pierson has suggested that the intense racial feeling in the South is due to the Civil War and, more especially, to the racial conflict during Reconstruction.[19] Undoubtedly, these various cultural and historical factors have played an important rôle in differentiating the course of the development of race relations in the two countries. They should not, however, be regarded as causative factors, but should be considered in relation to more fundamental ecological, economic and cultural factors.

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[14] Donald Pierson, "Negros in Brazil," (Chicago, 1942).

[15] *Ibid.*, pp. 128-129.

[16] *Ibid.*, pp. 131-132.

[17] See E. Franklin Frazier, '"The Negro family in Bahia, Brazil," American Sociological Review. 7 (August, 1942), pp. 467-468.

[18] See Helen T. Catterall, Editor. "Judicial cases concerning American slavery and the Negro. (Washington, D. C., 1926.) 1, p. 77.

The first fact of importance is the difference in the geography of the two countries. A large part of Brazil, especially the areas into which Negroes were imported, is in the tropics. Although the question has not been settled whether the white man can establish permanent settlements in the tropics, this factor has important consequences for race relations. The Portuguese, who have been regarded "particularly fitted for tropical settlement by environmental experience and racial history,"[20] were confronted, as other white races, with the problem of adapting themselves to life in the tropics. The most successful white settlement, at first by the Portuguese and later by the Italians and Germans, were in the highlands of Brazil. In the northern part of Brazil, where large numbers of Negroes were imported, there was a struggle for existence in a geographical environment in which the Negro had the advantage. The Negro slaves found their new habitat very similar to their homeland, in regard to the climate and the vegetation. It has been suggested by some Brazilian scholars that the ability of the Portuguese to survive in the new environment was due to the admixture of Negro blood. Whether this is true or not, in the southern States the white man was not confronted with the problem of meeting the superior adaptation of the Negro to the geographic environment. If it is true that Negro slavery displaced white servitude because it was more economical, this only emphasizes the fact that social rather than biological factors were responsible for the growth of the Negro population in the South. Even in those States where the plantation system has flourished, there has been a decline in the relative numbers of Negroes during the present century.

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[19] See Pierson, *op. cit.*, p. 347.

[20] A. Grenfall Price, "White settlers in the tropics," (New York, 1939), p. 15.

The next factor of importance in comparing race relations in the two countries is the economic relations of the two races. In Brazil, as in the southern States, Negroes were introduced primarily to provide the labor needs of the plantation economy. But the rôle of the plantation system in the economy of the two countries was different. Up until the nineteenth century, Brazil was essentially a rural civilization resting upon the plantation economy. In the United States, despite the political powers of the planter class, only eighteen per cent of the farms in the South were classified as plantations. Because of the romance that has grown up about the planter class, the white gentry has been absorbed with the "poor whites." During the period of the plantation regime in Brazil, there was neither a white gentry nor a "poor white" class of any importance from the standpoint of numbers. Moreover, the plantations were more self-sufficient than those in the United States. The dependence of the Portuguese upon the labor of the Negro was greater. This dependence was not simply upon the physical strength of the Negro. The Portuguese were dependent upon the technical skills of the Negro. Many of the Negro slaves introduced into Brazil were not only highly skilled craftsmen but were often more literate than the Portuguese. This was especially true of those who had come under Mohammedan influences.

Writing of the "great house" in which the owner led a patriarchal existence, Gilberto Freyre states that it "came to symbolize not only an economic but a social and cultural system. It served not only as the residence of the plantation owner, but as fortress, school, guest house, church, hospital, bank and even harem."[21] This description calls to mind many of the characteristics of the plantation system in the antebellum South. However, in the South, the plantation culture did not dominate society to the extent that it did in Brazil, nor were these various characteristics as highly developed as in Brazil. Although there was a tendency in the South for the plantations to assume a patriarchal character, women were not subordinated to the extent that they were in Brazil, where they "were treated by their husbands in an almost oriental or Moorish way."[22] Moreover, it does not appear that, under the patriarchal regime in the South, there developed the solidarity of feeling and sentiment that characterized the patriarchal household in Brazil. This was probably due partly to differences in racial attitudes, but it was also related to greater isolation of the Brazilian plantations. Even in regard to the racial mixture which occurred on a large scale in the ante-bellum South as in Brazil, there were important differences. It is likely, that because of Puritanical mores and the higher status of women, there was a sense of sin and greater protest against concubinage in the South. Attitudes toward racial mixture in Brazil had been influenced to some extent by the fact that many of the early settlers were men without families. They had mated with both Indian and Negro women, and the offspring of these alliances had provided wives for later settlers. On the other hand, the migration of family groups and white women to the colonies in North America tended to preserve the integrity of the white family.

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[21] "Some aspects of the development of Portuguese America," p. 83.
[22] *Ibid.*, p. 97.

As one studies the history of the relations between whites and Negroes during the plantation and slavery regime in the two countries, other important facts become apparent. In both countries, a class of free Negroes, among whom there were many mulattoes, grew up outside the slave system. In both countries these free Negros and mulattoes had gained their freedom through purchasing it, through the humanitarianism of their owners and, more especially, because their white fathers had set them free. But here the similarity between the situation of the free Negroes in Brazil and the United States ends. The free Negro in the South, and even in the North, was an anomaly. There was no place for him in the economic or social organization. In the North, he led a precarious existence, because European labor was plentiful and could exclude him as a competitor. In the South, especially in the plantation area, the free Negro could find no place in the economic organization and was considered a threat to the slave regime. Hence, free Negroes were concentrated largely in cities which have always provided a refuge for those elements that can not fit into the traditional social organization. In Charleston, South Carolina, and New Orleans, large and prosperous communities of free Negroes, largely of mulatto origin, grew up. Although, economically, they acquired a secure foothold in the economic organization, they constituted a distinct caste in society. Yet the continual sexual association between whites and the mixed-bloods tended to erase the color-line and nullify the attempt to maintain a pure white caste. In Maryland and Virginia, where the ecological basis of slavery had disappeared, a semifree class of Negro laborers came into existence. Sexual relations between this class and the whites were likewise making it impossible to maintain a caste system based upon race purity. Moreover, it should not be overlooked that, despite the effort to maintain caste in the southern States, white fathers often gave their children a good education and left them property which enabled them to rise in the world.

Yet, slavery in the United States provided a form of accommodation between the two races that did not exist in Brazil. This was not due entirely to the absence of race feeling in the latter country but also to the existence of a large "poor white" class in the United States. Even during the slavery period, there were protests on the part of the non-slave-holding whites against the monopoly which the free blacks and mulatto artisans had in the economic organization. As long as slavery continued and political power was in the hands of the plantation owners, who often hired out their black mechanics, the protests of the "poor whites" were ineffectual. However, when the slaves were emancipated they were thrown into competition with the "poor whites," who, through organization and a system of apprenticeship, were able to exclude the Negroes from competition. Moreover, when the industrial revolution reached the South, the new forms of industry demanded skilled industrial workers and not workers possessing the skills required in the older handicrafts. The cotton mills, which symbolized the impact of the industrial revolution upon the South, became an exclusive field of labor for the "poor whites."

These economic, political, and social conditions did not exist in Brazil. The transition from slave to free labor caused, to be sure, some dislocations in the economy. However, the abolition of slavery dealt the final blow to the decaying plantation economy and the patriarchal regime. In the shift of political and economic power and cultural dominance to the towns, the educated mixed-bloods, as well as the educated whites, rose to positions of importance.[23] There was, undoubtedly, some prejudice against the mixed-bloods, but they were too numerous and too powerful for the relatively small number of pure whites to exclude them from competing for a place in the political and social organization. Many of the mixed-bloods were officers in the army, which comprised many blacks as well as mixed-bloods. Therefore, the entire structure of Brazilian society, both from a racial and an economic standpoint, was such as to preclude the possibility of a bi-racial organization. Moreover, whereas in the United States abolition and the emergence of the non-slave-holding whites tended to sever the sentimental ties that had existed between the two races during slavery, in the Brazilian abolition the bonds that had grown up between the whites and the blacks and the mixed-bloods were never broken. In fact, it was often the colored sons of the aristocracy who won the prizes in the competitive life of the new commercial urban economy.

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[23] Freyre, "Sobrados e Mucambos," pp. 302 ff.

There is one phase of the contrast in race relations in the two countries that has been neglected until recent years. That is the difference in the extent to which the African culture of the Negro slaves survived in Brazil and in the United States. Within recent years, Professor Herskovits has been engaged in discovering African survivals among the Negroes in the United States.[24] Although, in certain isolated areas as, for example, on the coast of South Carolina and Georgia, a number of linguistic survivals have been discovered,[25] there is practically no evidence that African survivals have influenced the social development of the Negroes in the United States. Even Professor Herskovits, who makes the extreme claims for African survivals, states that "one can set off the United States from the rest of the New World as a region where departure from African modes of life were greatest, and where such Africanisms as persisted were carried through in generalized forms, almost never directly referable to a specific tribe or area."[26] On the other hand, in the case of Brazil, it is not necessary to engage in speculation concerning African survivals. First, it should be noted that the Negroes introduced into Brazil did not suffer the destruction of their social life nor experience the distintegration of their cultural heritage to the same extent as the slaves imported into the United States. Therefore, on the large plantations in northern Brazil and even in the region about Rio de Janeiro, it was possible for them to re-knit the threads of their social life and re-establish their traditional culture. In fact, because of the close commercial relations between Bahia, the chief port of entry for slaves, and Africa, diplomatic relations were established with Dahomey.[27] The most important African culture that took root in Brazil was the Yoruban, and it seems that it absorbed or displaced the Bantu and other cultures. For a time, however, the culture of the Islamized Negroes from the Sudan exercised considerable influence on the slaves and race relations in Bahia. It was the Islamized Negroes who were responsible for the recurrent insurrections in Bahia from 1807 to 1830 and the revolution in 1835.[28] The Islamized Negroes organized communities and established schools and, in fact, possessed more literacy and technical skill than the Portuguese adventurers, criminals, impoverished noblemen and gypsies who settled in Bahia.

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[24] Melville J. Herskovits, "The myth of the Negro past (New York, 1941).

[25] *Ibid.*, pp. 276-279.

[26] *Ibid.*, p. 122.

[27] Arthur Ramos, "Las Culturas Negros en el Nuevo Mundo " Version española (Mexico 1943), p. 265.

During the last decade of the nineteenth century, Nina Rodrigues began his pioneer studies of African cultural survivals in the religious practices and the folklore of the Negroes in Bahia.[29] With the exception of Manuel Querino's study of African customs in Brazil, the scientific study of the Negro did not engage the attention of scholars for twenty years after Rodrigues' work. Around 1926, Brazilian scholars, among whom Arthur Ramos, Gilberto Freyre and Edison Carneiro played leading roles, took up the work of Rodrigues. In their work, they have directed their attention chiefly to African survivals in folklore and religion. However, Freyre has devoted his attention not so much to African survivals among Negroes as the influence of African culture upon Portuguese culture in America. Interest of Brazilian scholars in both of these aspects of the influence of African cultural survivals on Brazilian society indicates their importance in the assimilation of the Negro.

The Negro in Brazil, in contrast to the Negro in the United States, resisted the process of acculturation. The uprisings on the part of the Islamized Negroes were neither a racial conflict nor a revolt against the treatment of their white masters. These revolts were essentially a continuation of the wars of the Mohammedans against Christians. Resistance to Christianity has not been confined to the Islamized Negroes. Islamized survivals in the religious practices of Negroes have almost entirely disappeared, whereas the Yoruban religious conceptions and practices have persisted until the present day. In the candomblés of Bahia and the shangôs in the northeast, many of the African elements have been preserved, though they have become fused with Christian ideas and practices. Before the African communities were broken up, other phases of African traditions and social organization persisted, and the relations between these Negro communities and the Portuguese community were similar to those between immigrant communities in the United States and the larger society. On the other hand, because of the advantages that the Negro enjoyed in the struggle for survival in the tropical environment, the Portuguese took over from the Africans many elements of the culture of the latter. African influences are apparent in the language, the diet and music of the Brazilians. The influences are not regarded as quaint or exotic excrescenses but as an integral part of the culture of Brazilian society.

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[28] *Ibid.*, pp. 277-279.
[29] See Nina Rodrigues. "Os Africanos" 2a. (São Paulo, 1935).

A rough index to the acculturation of the Negro and his assimilation into Brazilian society is provided in the study of the changing color of the population of Bahia and the relation of color to the occupational class structure. Whereas, according to the estimates of the statistician in the Department of Health, the proportion of whites in the population had remained about 33 per cent from 1897 to 1938, the black element had declined from 38 to 19 per cent and the colored element had increased from 29 to 47 per cent.[30] Just as we have seen that the designation "white" in the records of the Gabinete de Identificação did not mean pure white ancestry, the percentages for whites just cited include mixed-bloods. In fact, the term "white" is used so loosely in Bahia, which is called A Velha Mulatta or "The Old Mulatto Woman," that Brazilians often speak of the Bahian whites. The term "white" is closely tied up with the social status, as it has been in the case of mixed-bloods throughout the history of Brazil. Therefore, in considering Pierson's tables showing the proportion of whites, colored, and blacks in the various occupations, this fact should be kept in mind. According to Pierson, "The whites, as might be anticipated, are concentrated in the upper levels. Their numbers, both absolute and relative, diminish sharply as one descends the occupational scale, and they appear only in small percentages in the lower tiers."[31] For example, we find street-sweepers distributed as follows, as to color: mulatto, 62.7 per cent; black, 34.7 per cent; and white, 2.6 per cent. On the other hand, physicians show the following distribution: white, 63.0 per cent; mulatto, 20.0 per cent; black, 1 per cent; and branco da Bahia or Bahian white, 16 per cent. Pierson is correct, in my opinion, in ascribing the situation to the fact that the blacks have not been free as long as the mixed-bloods. By that statement, he means, of course, that because of their short period of freedom, their poverty, and their lack of education, the blacks as a group have not been able to compete with those of a lighter complexion.

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[30] See Diagram 1, in Frazier, "The Negro family in Bahia, Brazil," *loc. cit.*, p. 467.
[31] Pierson, *op. cit.*, p. 178.

But it would be wrong to assume that, during the process in which the Negro and the mixed-blood have been assimilated into Brazilian society, or even today, race and color consciousness on the part of the various racial elements has been entirely absent. I think the process of assimilation may be shown in a study of the lives of Machado de Assis and André Rebouças. The first was, perhaps, the greatest writer Brazil has produced, and the second, a distinguished engineer and an abolitionist. Machado was born in 1839 of poor mulatto parents, both of whom were free, his father being a house painter and his mother a laundress.[32] He was taught to read and write by his stepmother and later he secured a job in a printing office. When he went to Rio de Janeiro he entered upon the career of a journalist. For a brief period his creative efforts were in the field of poetry. But he soon turned his attention to the romantic novel, a field in which he established himself as a master of style and the author of the literary emancipation of Brazil from Portugal. Rebouças was born a year before Machado, of parents who had a small admixture of white blood.[33] His father was a lawyer and a provincial deputy who, because of his opposition to the smugglers of slaves in the State of Bahia, was forced to move to Rio de Janeiro. Young Rebouças, as well as his brother, received a good education in engineering and was sent abroad by his father to complete his education. He served as an engineer in the army during the war with Paraguay and surveyed the ground for the first important railways in Brazil. After an active career as an abolitionist, he followed the royal family into exile.

The first fact that strikes us in the careers of these two men is that they were able to compete on almost equal terms with white Brazilians. I say "almost equal terms," because Rebouças complained in his biographical notes that he did not receive the traveling fellowship to study in Europe because of his color. Yet, Machado was given opportunity to participate in the literary movement of his day and Rebouças was given every opportunity to utilize his education and talents as an engineer. Both of these men thought of themselves as Brazilians, though both had some consciousness of their racial origin. Machado was conscious of his mulatto origin and disliked any reference to it. Moreover, when he began to court the daughter of an aristocratic Portuguese family, there was considerable opposition on the part of her family. Yet, after they were married, they were accepted into the highest social and literary circles in Brazil. Despite the consciousness of their racial origin, they were not "marginal" men or cultural hybrids in the sense in which Park defined the term.[34] The reason for this was doubtless that there was no segregated colored community with which they could de identified. This had important consequences for their work as well as their personalities. Neither of these men were concerned with the race or color problem. Their work was a part of the culture of the community and it was evaluated in terms of the standards of the Brazilian culture. These men were not considered a "Negro writer" and a "Negro engineer," but Brazilian workers in their various fields. This is quite different from the situation in the United States, where there are Negro writers, journalists, and even biologists and chemists and a different standard for evaluating their achievements.

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[32] Lucia Miguel-Pereira, "Machado de Assis" (Rio de Janeiro, 1939).

[33] Ignacio Jose Verissimo, "Andre Rebouças" (Rio de Janeiro, 1939).

There is, in Brazil, little discussion of the racial or the color situation. It appears that there is an unexpressed understanding among all elements in the population not to discuss the racial situation, at least as a contemporary phenomenon. Apparently, there is a general recognition that Brazil is essentially a country of mixed-bloods and that a new ethnic type is being formed. Oliveira Vianna is a rare exception among the scholars, in that he regards both Indians and Negroes as inferior races and believes that, through Aryanization or a whitening process, Brazil will become a white nation.[35] Although there is no race problem in Brazil, the upper classes are conscious of color differences and these color differences become the basis of social distances that are maintained by a subtle system of etiquette. In fact, these distinctions would escape the casual observer and, even when one discovers them, it is dangerous to generalize about them. If one should suggest an American situation analogous to the situation in Bahia, one might point to the colored community in Charleston and New Orleans, forty or fifty years ago. Bahia is essentially a mulatto community where persons of light complexion tend to dissociate themselves as much as possible from those of dark or black complexion. Of course, this does not preclude friendships between whites and blacks. According to a lawyer of pure Portuguese ancestry, members of his group are likely to be more free and friendly with blacks than the mixed-bloods. It is not unusual for whites or so-called whites to marry people of light-brown and brown complexion. In such cases, the brown-skin people are likely to become essentially white persons, or at least the children of such unions would be regarded as white. In fact, one of the reasons that it is impossible to draw a color line, not only in the north but in other parts of Brazil, is because it would cut across families.

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[34] See Robert E. Park, "Human Migration and the Marginal Man," The American Journal of Sociology, **33** (1928), pp 881-893

[35] Oliveira Vianna, "Evoluçao do Povo Brasileiro" (Rio de Janeiro, 1933).

The prejudice toward black persons seems to operate most strongly in intimate social relations involving marriage and in the new type of social life which is developing in clubs and hotels. For example, black persons do not attend the weekly dances at the large hotels patronized by Brazilian officials and business men as well as foreigners; nor are black men to be found at the tennis clubs and the yacht clubs. They may attend on some special occasion, but they do not move about freely and they would not be invited to become members. Of course, if they marry a white person, their children would be eligible for membership provided their parents belonged to the upper economic classes. Since foreign whites frequent the hotels and the clubs, it may be asked, to what extent are they responsible for these attitudes toward the blacks? Undoubtedly, the British and the Americans would not care to have black people in these places and some Brazilians are sensitive to their attitudes in regard to blacks. But the foreigners are not entirely nor primarily responsible for the attitudes toward blacks. They reflect the attitudes of the mixed-bloods who seek to identify themselves with the whites. What has been said so far applies to the upper social and economic strata in Bahia. Color distinctions and prejudices against the blacks are seemingly absent on the whole from the mind of the masses. This is apparent not only in their everyday activities but in the numerous festivals where all colors mingle freely. In fact, it is among the laboring masses that race mixture is continuing on a large scale in Brazil.[36]

In the states São Paulo, Santa Catharina, and Paraná in southern Brazil, color prejudice is much more marked than in the north. These are the states to which have come large numbers of European immigrants—chiefly German and Italian—who have a different attitude toward the Negro from that of the Portuguese. In these areas, the Negro, especially the dark or black person of Negro descent, has become a conspicuous minority in a predominantly white population. In this region, the attitude toward the Negro assumes often the character of race prejudice as opposed to color prejudice. Although there are no legal discriminations against persons of Negro blood, they are isolated and discriminated against in subtle ways. From conversations with some of the leaders among the blacks, it appeared that the Negro suffers chiefly from the economic competition of the European immigrant, especially the Italian. As the Negro is pushed down in the economic scale, he is unable to acquire the education and the skills which would enable him to compete successfully with other groups. The only escape for the Negro is to mingle his blood with that of the whites. This he is doing, though not as freely as in other parts of Brazil.

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[36] Freyre, "Some aspects of the social development of the Portuguese in America," pp. 102-103.

Because of the isolation of the Negro in the south, a number of Negro organizations have come into existence to fight discrimination on the basis of color. Out of these various organizations has come *Frente Negra Brasileira,* which was started in 1931 as a movement to include all Negroes in Brazil.[37] In this year, the organization announced, at a meeting of over a thousand Negroes, its program for the improvement of the moral, educational, economical, and political status of the Negro. It was recognized by the government as a political party but, after the changes in the government in 1937, it retained only the cultural and social features of its program. Besides the *Frente Negra Brasileira,* a number of other Negro clubs and associations have been organized in the south and in other parts of Brazil. The organizations in the south are sharply differentiated from those in the north. In the south, they are fighting discrimination and are seeking to integrate themselves into the social and economic organization. On the other hand, in the north, they have cooperated with whites in studying the cultural contributions of the Negro and have fought for religious liberty for Negro cults, as well as the improvement of the social condition of blacks. It appears that the Negro organizations in Brazil lack the drive and motivation of similar organizations in the United States. This is doubtless due to the fact that racial discrimination is not as strong even in southern Brazil as in the United States. In São Paulo, where a number of these movements originated, there are two Negro professors on the law faculty of the University. It appears that amalgamation will constantly undermine these Negro movements unless outside influences affect present tendencies.

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[37] See Ramos, "The Negro in Brazil," pp. 167-176, concerning the various Negro organizations and movements in Brazil.

Outside influences certainly have had some effect upon attitudes toward colored and black people. Many Brazilians are conscious of being regarded as a colored nation by Argentina and other so-called white nations of South America. Then, in recent years, Nazi racial theories have had some influence among the Germans in the south who tended to intermarry with the Brazilians.[38] The most important source of outside influence has been the financial and industrial penetration of the country. The British and the Americans draw a color line, not only in their social contacts with Brazilians, but when their business houses employ Brazilians as white collar workers. Americans who have gone to Brazil as technical advisers have insisted that even distinguished black officials be ejected from hotels and, when their wishes were not respected, they have left the hotels. In spite of the "good neighbor" policy, it is likely that increasing financial and industrial penetration of Brazil by Americans will accentuate discrimination on the basis of color. Even at the present time, Brazilians are careful to select pictures of the right complexion for the American public.

In spite of these differences between the racial situation in Brazil and the United States, it should be pointed out by way of conclusion that the development of race relations in the two countries reveals some underlying similarities. In both countries, the close association of the whites and blacks produced a class of mixed-bloods. Although, in the United States, an attempt to maintain a caste system has prevented the identification of the mixed-bloods with the whites; through the process of "passing," persons with Negro blood have passed into the white race. With the increasing mobility of our population, it is likely that this will continue. It is hardly probable that the so-called "race purity" laws of Virginia and Georgia will stop the process. Race mixture in the United States, as in Brazil, has been one of the chief factors in the social differentiation of the non-white population, and it has facilitated the social mobility of colored individuals. The relation of color differences to occupational structure in Brazil closely parallels the same phenomenon in the segregated Negro community in the United States. Moreover, as in Brazil, this phenomenon in the United States is a rough index to the process of acculturation, though it does not lead to complete assimilation, because of the attempt to maintain a racial caste in the United States. As the attempt to maintain a caste system becomes less effectual because of urbanization and the general educational and cultural development of the Negro, it is likely that the racial situation will approximate the situation in Brazil.

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[38] Emilio Williams, "Assimilação e Populações Marginais no Brasil," São Paulo, 1940, pp. 76-166.

NEW MEMBERS

Elected April 27, 1944

SUSTAINING MEMBERS

Auchincloss, Hugh, M.D., Surgery, Professor of Clinical Surgery, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Barzilai, Gemma, M.D., Pathology, Specialist in Obstetrics and Gynecology, New York, N. Y.

Robinson, Cortland O., M.D., Medicine, Physician, Pan American Airways, New York, N. Y.

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Berk, Bernard, Ph.D., Chemotherapy, Lawrence Richard Bruce, Inc., Stamford, Conn.

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Bull, Nina, Biological Psychology, Research Associate in Psychiatry, College of Physicians and Surgeons, Columbia University, New York, N. Y.

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deGara, Paul F., M.D., Bacteriology and Immunology, Instructor in Pathology, Cornell University Medical College, New York, N. Y.

Friedeman, Ulrich, M.D., Bacteriology and Immunology, Chief of Division of Bacteriology, Brooklyn Jewish Hospital, Brooklyn, N. Y.

Greep, Roy O., Ph.D., Biology, Associate in Pharmacology, Squibb Institute for Medical Research, New Brunswick, N. J.

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Rosenfeld, Capt. Morris, M.D., Pharmacology and Experimental Therapeutics. Now—Captain, Army of the U. S. (on leave); Associate in Pharmacology and Experimental Therapeutics, Johns Hopkins School of Medicine, Baltimore, Md.

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Watkins, William Way, Ph.D., Chemistry of High Polymers, Research Supervisor, Rayon Department, E. I. DuPont de Nemours & Company, Buffalo, N. Y.

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Worley, Leonard George, Ph.D., Cytology, Associate Professor of Biology, Brooklyn College, Brooklyn, N. Y.

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Turkevich, John, Ph.D., Physical Chemistry, Assistant Professor, Chemistry Department, Princeton University, Princeton, N. J.

Wannier, Gregory H., Ph.D., Physics and Chemistry, Lecturer, State University of Iowa, Iowa City, Iowa.

STUDENT MEMBERS

Solomon, Nathan A., B.S., Biochemistry, Student, Polytechnic Institute of Brooklyn, Brooklyn, New York; Chief Administrative Technician, Jewish Hospital, Brooklyn, N. Y.

Stein, Marcella, Psychology, Junior Psychologist, Brooklyn College, Brooklyn, N. Y.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 6 JUNE, 1944 No. 8

SECTION OF GEOLOGY AND MINERALOGY

MAY 1, 1944

DOCTOR CHARLES S. DENNY, Wesleyan University, Middletown, Conn.: *Glacial Geology along Alaska Highway from Dawson Creek, British Columbia, to Whitehorse, Yukon Territory.*[1] (This lecture was illustrated by lantern slides.)

The Highway follows the western edge of the Alberta Plateau from Dawson Creek northward to Fort Nelson, British Columbia, a distance of about 300 miles. A subsequent lowland separates remnants of this plateau from the foothills of the Rocky Mountains to the west. The contact between Rocky Mountain drift and Keewatin drift lies in this lowland. The Rocky Mountain drift lies beneath the Keewatin drift and the two are separated by lake deposits. The Keewatin drift is slightly weathered to a maximum depth of eight feet.

At Summit Lake in the northern end of the Rocky Mountains about 80 miles west of Fort Nelson, there is a bouldery, lime-cemented till probably of pre-Wisconsin date. At the earliest stage of the last epoch of mountain glaciation in the Summit region, glaciers extended far down the valleys to elevations below 3500 feet. The three (?) recessional stages of these valley glaciers are probably of late Wisconsin date.

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[1] These observations were made during the summer of 1943 when the author was a member of an expedition led by Dr. H. M. Raup of the Arnold Arboretum, Harvard University, under the auspices of the Joint Economic Committees, Canada-United States. The geological work was financed by a grant from the Penrose Fund of the Geological Society of America.

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The Liard River above the Grand Canyon follows a narrow flood plain incised below a kettled outwash terrace. The headwater region of the Liard near Watson Lake is a broad lowland of bouldery till-covered hills and outwash plains dissected by the Liard and its tributaries. The large lakes and rivers in the Teslin to Whitehorse district are surrounded in part by outwash plains and lacustrine silt and clay. These deposits were laid down around wasting valley glaciers.

Various types of mass movements, notably slumps or debris slides, are characteristic of many slopes especially in the region east of the Rocky Mountains.

Stone-rings and stripes and related structures resulting from frost action were described from the Summit Lake area, from the Alberta Plateau near the Beatton River and from peaks between Squanga and Little Atlin Lakes. The relationship of the vegetation to these various soil structures suggests that the former has gained a foothold on surfaces which were more actively frost-heaved in the recent past.

The various types of vegetation and the glacial deposits were mapped on aerial photographs, and some probable relationships between the soil and the vegetation were noted.

SECTION OF BIOLOGY

MAY 8, 1944

DOCTOR DEAN BURK, MARIE L. HESSELBACH, DORIS F. MACNEARY, and DOCTOR RICHARD J. WINZLER, National Cancer Institute, National Institute of Health, United States Public Health Service, Bethesda, Md.: *The Vitamer Concept.**

As science and the sciences develop, it is rewarding to recognize and acknowledge early the new concepts, principles, and criteria that constantly emerge from new observations and data, and to crystallize such generalizations with appropriate nomenclature that will be found convenient and useful by those who are confronted with the need to deal with and express the ideas back of the terms. Experience shows that realization of the various general possibilities and interrelationships covered by and anticipated by such new terms greatly accelerates the development of suggestions for new experiments and applications that would otherwise be arrived at far more slowly and laboriously and less systematically. Although much terminology will be proposed in what follows, the primary interest will naturally rest in the underlying concepts outlined.

Vitamers. The rapidly developing recognition of the multiplicity of chemical compounds that possess a specified vitamin activity has created a scientific need for a term that will indicate a biological relationship between all such compounds, naturally occurring or synthetic. The term *vitamer* has been proposed[3, 4, 5, 6, 7, 13, 24a] to refer to any one of a number or group of chemically related nutrient compounds that act to overcome a given vitamin deficiency in one or another organism, plant or animal. The degree of chemical relationship need not be very close: that existing between biotin and pimelic acid could suffice, as would also isosteric relations; but quantitative synergistic relations between members of different vitamer groups, such as occur between vitamin A and vitamin E, are not ordinarily implied. Each group of vitameric compounds may, for convenience, be named with reference to the initially established vitamin whose activity defines the group. Thus, there are now many known biotin vitamers, niacin vitamers, thiamin vitamers, pyridoxin vitamers, A vitamers, D vitamers, E vitamers, K vitamers, etc.

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* This paper covers the conceptual aspects of an illustrated lecture delivered by Doctor Dean Burk before the Section of Biology, under the title of "Biotin Vitamers in Metabolism." The lecture included a large amount of experimental data, too extensive to be given here, on biotin vitamers in assay, growth, metabolism, and synthesis by lower and higher organisms.

It follows from the foregoing that all vitamers are either vitamins* or vitamin precursors, but a particular vitamin becomes classifiable as an actual rather than potential vitamer only after at least one other compound with the same type of activity has been discovered. The term vitamer possesses essentially all of the connotations and attributes of the term vitamin, but, in addition, it serves to classify vitamins into group relationships based upon specified *activities.* The term vitamer is intended to refer primarily to biological activity or function, rather than to a chemical formula or formulae; for molecular structure alone cannot—by the principle of holism—account fully for biological function, any more than can other chemical or physical properties of molecules or substances, e.g., oxidation-reduction potential, polarographic (mercury electrode) potential, dielectric constant, acid-base dissociation constant, radiation absorption constants, optical rotation, density, melting point, boiling point, specific heat, etc.

After being taken into a living system, it may or may not be necessary for a given vitamer to undergo further chemical change before becoming active. Whether active or inactive when administered, all related substances which ultimately satisfy a given vitamin requirement are vitamers (vitameric). The active form or forms in the organism may well differ from the form supplied in the diet, as in the case of administered thiamin that is converted in an organism into the active coenzyme thiaminpyrophosphate (cocarboxylase). In the majority of cases studied so far, however, it would appear that various vitamers satisfying a given requirement are usually converted into the parent or prototype vitamin by which the vitamer group was first recognized. Thus, miotin, tiotin, and the diaminocarboxylic derivative of biotin (DAC), all avidin-uncombinable vitamers of biotin, have been shown[4] to be converted during utilization of *Saccharomyces cerevisiae* Fleischmann strain 139 into an avidin-combinable vitamer as yet undistinguishable from biotin; likewise, desthiobiotin, an avidin-combinable vitamer of biotin, has been shown[8] to be converted by this strain of yeast to a vitamer that is presumably biotin. The conversion of beta-carotene and other provitamin A compounds to vitamin A, which is stored in the liver, is also a familiar example.

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* A vitamin is an organic compound required in small amounts in the nutrition of a given organism otherwise supplied adequate amounts of protein, carbohydrate, lipid, and minerals. It is recognized as a vitamin by its biological activity in overcoming a particular deficiency or syndrome. A cardinal feature of a vitamin is that it must be a nutritional component, i.e., it must be supplied to the organism. Its mere occurrence in an organism is not a sufficient or even necessary criterion of its being a vitamin for the organism, since the organism may have synthesized it as a by-product and may not be making any use of it. Although the chemistry and internal physiology of vitamins, hormones, enzymes, and other similar types of biologically active materials are now known to have many aspects in common, the distinctive nutritional aspect of a vitamin makes the retention of this term as necessary, and more useful, than ever.

It must be emphasized that in using the term vitamer, as in using the term vitamin, it is always necessary to keep the species requirement in mind, implicitly if not explicitly. For, just as in the case of any vitamin, a vitamer may be required (utilized) in the nutrition of one species but not in the nutrition of another, in which case it is not a vitamer or a vitamin for the latter species, even though it is for the former. Thus, desthiobiotin is utilized by, and hence a biotin vitamer for, *Saccharomyces cerevisiae* str. 139,[8, 20] 25 other strains of this species,[19] and many other organisms;[19] but desthiobiotin is not utilized by, and hence not a vitamer or vitamin for, *Lactobacillus casei*,[8, 19, 20] *Rhizobium trifolii*,[19] and various other organisms.[19] It must also be kept in mind that the existence of a species requirement, or the demonstration thereof, may depend upon particular conditions, even highly artificially imposed ones; thus administration of antivitamins may induce a vitamin deficiency, even where none appeared to exist before, as in the cases of avidin-induced biotin deficiency, isoriboflavin-induced riboflavin deficiency, pyrithiamin-induced thiamin deficiency, or sulfonamide-induced *p*-aminobenzoic acid deficiency. It is also well recognized that the vitamin requirements of an organism may be greatly modified quantitatively or synergistically[24b] (vitamin balance) by the nature of the rest of the diet, or even qualitatively by other environmental or hereditary (adaptational or mutational) factors.

The question of species response assumes especial importance in bioassay. A material being analyzed may contain vitamers not utilized by the assay organism; thus, human urine contains (in addition to biotin) the avidin-uncombinable miotin, which is not utilized by, or assayable with, or a vitamer (or vitamin) for, *Rh. trifolii*, although it is for *S. cerevisiae* str. 139.[4] On the other hand, the assay organism may respond to vitamers for it is present in, but not necessarily utilized by or a vitamer for, the organism being analyzed; thus, *S. cerevisiae* str. 139 responds to avidin-uncombinable biotin materials contained in animal tissues and tumors, in which tissues a definite vitameric function of such materials yet remains to be demonstrated or contraindicated. A careful experimental distinction between vitamers and entirely unrelated accessory growth substances that may also increase growth or function is necessary here.

That a compound is not active as a vitamer for some species (or under some conditions) does not preclude its classification as a member of a vitamer group with which it has already been identified by virtue of a given vitamin activity among certain other species. Obviously, it is not necessary for all members of a given vitamer group to be active in all species, any more than that a particular vitamin must be required by all species in order to be referred to, upon appropriate occasion, as a vitamin. Thus, even though ascorbic acid is not normally required in the diet of the rat, and is therefore not normally a vitamin (or vitamer) for the rat, the tissues of the rat contain ascorbic acid (as a result of synthesis), and this may be analyzed for and identified as a C vitamer by some other species which does require a C vitamer in its nutrition.

In this last connection, ascorbic acid may be referred to as an *autovitamer* for the rat, if it is synthesized by the rat's tissue, for it presumably performs the same function in the rat as vitamin C does in species that require it in their nutrition. Vitamins synthesized in the intestinal tract by microbial flora thereof are probably now best regarded as true vitamins (or vitamers) and not autovitamins (or autovitamers) for the host, since so far as the living host organism is concerned, the intestinal flora represent an independent source, and the same would be true of vitamin compounds similarly synthesized by any other parasite or symbiont located even more definitely within the host, as in the case of invading pathogens. Since the use of essentially sterile animals, free of micro-organisms, will probably shortly be a laboratory commonplace, it is probably best to anticipate this approaching experimental situation, when the view just expressed will also, as a consequence, be commonplace.

As a concluding reflection it may be pointed out that a compound may conceivably, by virtue of a multiplicity of chemical properties, be divitameric or multivitameric, i.e., belong to more than one vitamer group, just as methionine may serve to provide both methyl groups and sulfur in nutrition; or a synthetic compound might be prepared between niacin and biotin with activity as both biotin and niacin vitamers; the extreme case would be the compound that provided all vitamin activities, the universal vitamin!

Other -mer terms. At considerable risk of loss of simplicity, the usefulness, convenience, and derivation of the term vitamer can be further elucidated by pointing out that since the introduction of this word into the English language a number of corresponding terms, possibly of lesser immediate service, have arisen in consequence. Thus, a hormomer is any one of a number of chemically related compounds that possess a given hormonal activity[6, 7, 16] and, in the field of plant hormones, there are auximers,[2, 6] or epinastamers, hyponastamers, parthenocarpamers, etc. Any number of entities, biological or otherwise, that possess a particular activity, function, or purpose may be expressed by words of similar construction and implication ending in -mer*; and all such words may be conveniently used as certain other parts of speech, particularly the adjective form.

The word isotel has been proposed[25] to indicate a relationship between entities that have the same purpose (Gr. *telos*), but unfortunately this word does not specify *which* purpose. A -mer word, on the other hand, does state or imply—in its first part—the specific purpose or activity involved. Isotel is useful and good as a general term, but it is clearly inadequately definitive of, and no substitute for, a -mer term, when it is desired to indicate a particular activity or function (vitameric, hormomeric, auximeric). To utilize the term isotel to specify *which* purpose or activity, one would be constrained to employ such phraseology as vitamin isotel, hormone isotel, auxin isotel, which would become especially awkward were other parts of speech than the noun employed.†

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* The new -mer words have been made up of components that take their prime meanings from connotations of modern scientific words in *present-day English*, without regard to partial coincidental derivation that might be possible from roots existing in ancient languages or from any misplaced belief in the necessity for using unmixed roots thereof, a belief belied by numerous accepted scientific English words so derived. Full derivation from ancient languages is precluded in the present -mer instances by the principle of holism, according to which the whole is greater than the sum of its parts. Thus, etymologically, the word vitamer is not, in fact, coined as a combination of ancient Latin and Greek components meaning literally "life-part,"[17, 25] but actually took origin and development from the great bulk of implications of the modern scientific word vitamin, and from the usual implications invested in those modern scientific words in the English language by the ending -mer. The first parts of the -mer words, such as vitamers, hormomers, auximers, etc., usually involve contractions of words implicating purpose or activity, the degree of shortening of the etymon, or primary word giving rise to the -mer derivative, being governed as much by euphony as by any other consideration, by analogy, avidin represents a convenient and euphonious contraction of avidalbumin. Where it might be desired to take the name of any chemical vitamer compound of well-known activity as a point of reference, whether or not it is the first one of a given vitamer (or hormomer, etc.) group discovered, one might say niacinamer, biotinamer, miotinamer, desthiobiotinamer, etc., where this system does not become awkward with long chemical names. In any event, the suffix -mer (implying form of, part of, type of) indicates a group relationship based on a specified activity, function or purpose given by or implied by the first part of the word.

† To avoid this difficulty one might say vitatel, hormotel, auxitel, or biotinotel, miotinotel, niacinotel, etc., but this would be redundant since the first parts of these words already imply a given purpose or activity; and, moreover, such words are not as euphonious, especially in other parts of speech, as the corresponding words ending in -mer, where there is no such redundancy. Corresponding difficulties occur with isovitamin, isohormone, isobiotin, isoniacin, etc., though to a lesser degree, and the prefix iso- already has connotations in chemical literature referring, in such words as isotherm, isobar, etc., to aspects of entities that are actually identical.

Anti -mers. Just as the existence of various antivitamins is now well established, so there are antivitamers, antihormomers, antiauximers, etc., with equivalent significances. Specifically, an antivitamer is either (1) any antivitamin, nutritive or otherwise, that interferes with the activity of one or more members of a given vitamer group, or (2) any one of a group of antivitamins that interfere with the activity of a single vitamin for which there may yet be no known vitamers. An antivitamer might thus act against all known members of a given vitamer group, with all tested species, under all tested conditions; but, as a minimum for classification as an antivitamer, it need act only against one vitamer, in one species, under one particular set of conditions; or, if it is one of a group of antivitamins, then against only one vitamin, in one species, under one particular set of conditions. Whereas vitamers are vitamin forms that can replace one another, antivitamers include either antivitamins that can replace one another or a single antivitamin that interferes with any member of a vitamer group. An interesting example, among many that might be cited, is that of antivitamers inhibiting one vitamer but not another of the same group, as in the inhibition by beta-aminobutyric acid and isoserine of yeast growth in the pantothenic acid vitamer beta-alanine, but not (in the concentrations tested) in growth with pantothenic acid itself.[21, 22]

It is also to be pointed out that a given compound may be (1) vitameric with one species or under one particular set of circumstances, (2) antivitameric[*] with another species or under another particular set of circumstances, or even, (3) nonvitameric, or (4) nonantivitameric, as is clearly established by many experimental instances already observed: e.g.,[19] (1) desthiobiotin acted as a biotin vitamer for 25 strains of *S. cerevisiae* and also some 15 other organisms; (2) as an antibiotin (biotin antivitamer) for *L. casei, Sordaria fimicola,* and *Ceratostomella pini* 416; and (3) was inactive (at the concentrations tested) for growth of *Ceratostomella pini* 416, *Sordaria fimicola, L. casei, L. arabinosus,* and *Rh. trifolii* 205, in a biotin vitamer-free medium; and (4) did not act as an antibiotin for *L. arabinosus* and *Rh. trifolii* in medium containing biotin. In referring to or naming various antivitamers, one may speak of, e.g., biotin antivitamer, niacin antivitamer, etc., where it is not necessarily desired to specify which particular vitamer of a given vitamer group is acted against; but where it is desired to specify the actual vitamer of a given vitamer group acted against, one could say antibiotin, antibiotin methyl ester, antimiotin, etc.

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[*] When a given compound is both vitameric and antivitameric (under differing circumstances), it may be referred to as *amphi*vitameric, just as when a given compound is both carcinogenic and anticarcinogenic (under differing circumstances) it is termed amphicarcinogenic.[6] The existence of a number of demonstrable amphivitamers makes it desirable to weld the concept of amphivitamer into our experimental thinking and interpretation of the bewildering array of nutritional data at hand.

Three now generally accepted types or modes of antivitamin action apply likewise to antivitamer action: (1) competition, (2) combination, and (3) destruction. Thus, for example, in the case of biotin or certain vitamers of biotin, one has (1) (reversible) competition by desthiobiotin in certain species, (2) combination with avidin, and (3) (irreversible) destruction by H_2O_2 or other oxidizing agents.

It is important to note that a chief point of interest about antivitamers (and similarly with other types of antimers) is that they may create a vitamin need, even where none existed before, and that they may do this without calling for the extensive modification of the rest of the diet that is commonly required when a vitamin deficiency is established by its attempted omission from the diet, or by attempted prevention of intestinal synthesis. The relative specificity of antivitamer action provides a relatively specific elimination or blocking of the normal vitamin (vitamer) activity. This assumes importance, not only in academic studies, but also in therapeutic and industrial applications, where the course of disease, or the course of, say, fermentation, might be greatly altered in a desired and regulated manner. The possibilities in cancer studies for producing effective vitamin deficiency differentials between normal adult tissues and growing neoplastic ones, especially by means of the antivitamers that act competitively, have been discussed elsewhere[6]: "Rapidly growing tissue such as tumor presumably must synthesize its vitamin-containing enzymes from available constituents, and might thus have its new growth made up in part with inactive or competitive forms of vitamers that might be administered. This might result in the inability of the tumor to grow. The more stable normal tissues might have much less tendency to replace their active vitamers with the inactive forms and thus could be affected to a much lower extent than the proliferating tissue."

As concluding remarks, it may be stated that the concepts and nomenclature outlined in this paper have been advanced on the basis of ideas arising from informal discussion and consultation with many others, particularly at symposia and meetings such as the Gibson Island Conferences on Chemical Growth Factors and Vitamins held during the past two years, where, in fact, all of the actual -mer type terms cited first took origin or underwent announcement.* Whereas the vitamer terms may be regarded as adequately crystallized, the other types of -mer terms may be looked upon as logical extensions that must undergo further testing in regard to need and use by workers in the respective fields. It is obvious that all such terms deserve a place in language when and in so far as they are useful and serve a need. It is only on such a basis that they have been set forth here. Moreover, it is not wise to seek or anticipate realization of absolutely sharp definition, for, in the words of G. N. Lewis,[18] "The theory that there is an ultimate truth, although very generally held by mankind, does not seem useful to science except in the sense of a horizon toward which we may proceed, rather than a point which may be reached."

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* According to Hickman,[14] the word vitamer and its adjective "vitameric" were coined and used by him and his research associates for some time before their first verbal proposal by him in a discussion at the 1942 Gibson Island Vitamin Conference,[13] which proposal was made following conversation with one of the present writers as to which word—isovitamin, vitaminamer, vitamer, etc.—would best satify our respective and independently developed needs for a term to indicate group relationships within the A vitamins or E vitamins on the one hand and various compounds with biotin activity on the other. The dual origin of a need for some such term in separate research groups shows how necessary an expanded nomenclature had become for the proper advancement of vitamin research, and the same may be said, in regard to other fields, for the subsequent and consequent derivations by ourselves and others of hormomer[16] auxiner,[2] etc. A good description of the early origin and development of the term hormone, coined by W. B. Hardy, is given by Sir Wm. Bayliss,[1] and of the original term vitamin itself by Casimir Funk,[10, 11, 12] and of some of the early inertia regarding the term vitamin by an editorial.[9] In this connection, it is of interest that, according to Jacoby,[15] "In 1911, when Dr. Funk was working at the Lister Institute in London, he submitted several papers for publication to the head of the department who crossed out the word 'Vitamin' whenever it appeared. In 1912, Dr. L. Reichmann asked Dr. Funk to write a summary of his papers and ideas for the Journal of State Medicine, London.[10] This paper as a review did not have to be submitted to the Lister Institute before publication and therefore the word 'Vitamin' (then spelled 'Vitamine') passed."

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SECTION OF PSYCHOLOGY

MAY 15, 1944

DOCTOR WALTER FREEMAN, and DOCTOR JAMES W. WATTS, Department of Neurology, George Washington University, Washington, D. C.: *Behavior and the Frontal Lobes.* (This lecture was illustrated by lantern slides.)

The frontal lobes have excited the curiosity of scientists for more than a century, but they have excited the admiration of the sculptor, the painter and the poet variously for more than the duration of recorded history. For it was when man first began to look about him and to recognize the differences between himself and his fellows, and then to look more closely at the individuals he admired most, that he singled out the person with the broad and shining forehead as the epitome of everything noble. And whence did Athene spring, full panoplied, but from the brow of Jove?

The frontal lobes have received panegyrics and encomiums on the one hand, and disparagement and neglect on the other. From the first high-flown theories of Gall and Spurzheim with their super-localization down through the annihilating verbosities of Flourens and then up again with Bianchi and down with Pavlov, the frontal lobes have undergone a variety of treatment (one might almost say punishment) that has continued to the present. Perhaps Tilney has expressed most clearly the ennobling influences of the frontal lobes, while, even in our day, Lashley insists that brain function depends upon brain mass, and Kleinst and Goldstein are poles apart in dividing up the functions of the frontal lobes in accordance with cytoarchitectural differences. To return to Tilney: "Perhaps it is hazardous to define any single area in the brain as the region of supreme organization. . . . Yet it is reasonable to attribute a certain superiority to that neopallial region which is charged with the functional representation of all other cortical territories, which combines the highly particularized functions of all other areas in broader impressions of human existence, which acts as the accumulator of experience, the director of behavior, and the instigator of progress." And our friend Karpman says he will some day write a paper: "The Frontal Lobes; Enemies of Mankind." Which way lies the truth? Both ways.

We believe that we can best enter upon a discussion of the problem of behavior and the frontal lobes by presenting certain characteristic reactions of individuals who have resumed their place in society following prefrontal lobotomy for the treatment of disabling mental disorders. We can then see where these individuals fall short of the ideals of human behavior and also where they excel. By analysis of the various factors we may be able to determine what it is that is added to human personality by possession of intact *and harmoniously functioning* frontal lobes. Finally, by analysis of the same set of facts, we may be able to determine what handicaps, pitfalls and disasters may result from perverted activity of those same frontal lobes. Substantially, the frontal lobes would seem to occupy a place of supreme importance for human betterment; yet for the individual their possession may be an intolerable burden leading to suicide and psychosis. But first we may describe the operation of prefrontal lobotomy and what happens to the brain as the result of operation.

PREFRONTAL LOBOTOMY

The frontal regions of the scalp are shaved and sterilized, and the surgeon then marks the midline and the approximate location of the coronal suture. After incising the scalp vertically, centering about a point on this line 6 cm. above the zygoma, he retracts the tissues and places a burr hole through the coronal suture on both sides. He reflects the dura, secures hemostasis, punctures the cortex in an avascular area, and then, under guidance, measures the diameter of the frontal lobes by passing a long cannula from one opening in the skull through the genu of the corpus callosum and out through the hole on the opposite side. He then locates the sphenoidal ridge by angulating the cannula downward toward the base of the skull. When he has located this ridge by slipping over it into the middle fossa, his assistant notes the relationship of the cannula with the line drawn upon the scalp, and if necessary, this line is redrawn. The surgeon removes the cannula and inserts a blunt knife along the original tract of the cannula, and at a depth of 4 cm. or thereabouts, depending upon the width of the frontal lobes, makes a sweeping incision first upwards, then downwards, always maintaining the knife in the selected plane. After washing out the wound with saline to control what little bleeding may occur, the surgeon deepens the incisions by making radial stab incisions that bring the broad tip of the knife substantially to the midline, severing the remaining fibers in the upper and lower quadrants of the frontal lobes. He then makes his final irrigations and injects a few drops of iodized oil into the depths of the incisions and proceeds with the same technic on the opposite frontal lobe. He closes the incisions, bandages the head and marks upon the bandage the exact location of the burr holes so that the technician can accurately center the patient's head when making the roentgenograms. Satisfactory films are of great importance in determining the cause of failure of an operation. If the incisions are accurately placed but the patient's mental condition is unimproved, we conclude that even more of the frontal lobe tissue must be sacrificed at a second operation. If the incisions vary to any considerable degree from the plane selected, we may reoperate. If the incisions lie too far anterior; or, if the incisions run behind the ideal plane too far, we may prepare the relatives for a prolonged, difficult and incomplete convalescence.

The immediate postoperative course following prefrontal lobotomy is of some importance to the understanding of the functions of the frontal lobe. The picture is clouded, however, by the presence of a large number of phenomena that are probably more readily explained by diaschisis, the temporary throwing out of function of undamaged nerve cells when their incoming stimuli are abolished by severing their afferent connections. However, diaschisis is not the entire explanation. We prefer to operate upon patients under local anesthesia so that their conversation can be recorded. Even when three of the four quadrants are severed, the patients may still be able to carry on a coherent conversation in full contact with their environment. It is only when the fourth quadrant is sectioned, and sometimes only following the radial stab incisions, that their replies become irrelevant, difficult to obtain, and manifest complete disorientation. In explaining this phenomenon, we have supposed that the functions of alertness and orientation are exercised by the posterior portion of the frontal lobe, and that severing connections far back has the effect of throwing out of function enough of the cellular constellations lying anterior to the incisions to leave those lying posterior incompetent to maintain these rather basic functions of the personality. The farther forward the sections are made the less manifest are these alterations, whereas the farther back the incisions are made the longer the time elapses before liveliness and orientation are regained.

In addition to inertia and disorientation there are a number of other symptoms that are transitory, some of them disappearing rapidly, others slowly. Vomiting usually disappears in a day or two, even though it may have begun at the first incision on the second side. Vesical incontinence is very common in the early post-operative stage but usually disappears even after a very extensive operation. Masking of the face and plateau speech are usually observed following operation in the plane of the coronal suture, but not when the incisions lie anterior to this landmark. Such phenomena are prolonged when the fibers are severed back of this plane, and when the incisions lie far enough back it is not uncommon to observe reflex grasping and a positive Babinski sign. The farther back the incisions lie, the slower the return of normal function. Some patients do not utter a word for days or weeks after an extensive operation, not because of aphasia but apparently because of inertia. Eventually, they regain their language function, often rather suddenly and then completely, without the stages of recovery that one observes in aphasia.

Inertia may also take the form of perseveration, which is merely a repetition of the same act over and over again. We have saved some letters written by patients a few days after operation and have seen the same word or phrase repeated over and over again, line by line and page by page. Recently we watched a patient working out with a punching bag, striking it rhythmically 72 times a minute with the same hand, never missing and never changing his stroke, over a period of seven minutes before we finally distracted him sufficiently to get him astride a stationary bicycle. He then kept up rhythmic pedaling for another half hour, seldom varying the speed and with the same set expression upon his face. It was necessary to pull his feet from the pedals in order to make him stop for supper.

In contrast to the inertia that develops immediately following operation in the posterior parts of the frontal lobes, is the excessive liveliness and high spirits that occur if the operation is placed anteriorly. Sometimes, on the evening of operation, there is noted considerable restlessness and overactivity, singing and whistling, a sort of careless gaiety that contrasts strongly with the somber mood or anxious state that had prevailed preceding operation. This is now looked upon as an unfavorable sign, since the patient always relapses.

Reviewing these two opposite reactions, inertia on the one hand and excessive liveliness on the other, we might argue that the mechanisms underlying self-restraint are to be found in the frontal lobes, as well as the mechanisms underlying alertness and orientation. However, it would appear that whereas alertness and distractibility increase after an operation anteriorly and then disappear with a return of morbid symptoms, the restraining factors in the personality are mediated by cellular constellations lying in advance of those mediating orientation, particularly since orientation is unaffected by an operation in the anterior plane. Furthermore, with recovery from the inertia, there is apt to be a prolonged period of unproductive overactivity. As one man said of his wife: "She's just like a hummingbird, always on the go. If she'd settle down for a moment sometimes, she'd accomplish something, but she just can't seem to."

Frequently we have had occasion to observe the behavior of a patient following two or even more operations, each one severing connections progressively farther back. Obviously, secondary operations are much more damaging to the patient's capacity for social readjustment, but are necessary in order to interrupt the repetitive behavior characteristic of the psychosis. We recall vividly the case of a young obsessive neurotic who had gone through three operations on his frontal lobes with maintenance of his orientation, alertness and preoccupation. The third operation was limited to one side because of disarrangement of the drapes and hence could not be termed adequate. Following the fourth, there developed immediately a satisfactory degree of inertia, disorientation, masking of the face and plateau speech that made it certain that he would no longer be harassed. He slowly proceeded to make a fairly satisfactory recovery, but he persists in his unproductive overactivity.

We have not been able to determine whether there are any finer localizations in the frontal lobes. Even in our cases of most extensive transection, inertia has gradually given away to varied activity, and disorientation, masking and plateau speech have disappeared. Presumably, the functions of alertness and contact with the environment are carried out by widely distributed constellations of neurones located in the frontal lobes. The only definite indication that they are situated on the mesial surface comes from a case in which inertia, lasting four months until death, occurred in connection with laceration of the anterior cerebral artery, causing extensive cortical degeneration on the mesial aspect of the hemisphere.

BRAIN CHANGES

Before discussing other aspects of the personality following prefrontal lobotomy, we shall demonstrate what happens to the brain that has been operated upon. We have several examples in which the evidence is corroborated from different points of view, pneumoencephalography, electroencephalography, and necropsy.

The pneumoencephalogram shows moderate to marked dilatation of the anterior horns of the ventricles, sometimes with a porencephalic cyst if the incisions happen to penetrate into the ventricles. There is also a moderate increase of air over the frontal lobes of the brain.

The electroencephalogram varies considerably in different cases and at different periods following operation, but in cases in which the recovery is satisfactory there is apt to be a normal wave pattern. Sometimes, in the early postoperative stages, there are large slow waves, and these would seem to be more marked when the incisions are made far back and in which the caudate nucleus is probably traumatized.

Necropsy studies reveal no gross or microscopic abnormality of the frontal cortex with the exception of the areas directly trespassed upon by the knife. There is no obvious degeneration in the myelin sheaths posterior to the lesions, but there is some degeneration in Marchi preparations anterior to the incisions, chiefly in the superior portions of the frontal lobe. The cysts remaining after operation are clean-cut, with shiny walls, very little fibrosis or gliosis in the vicinity and no significant inflammation, even in the immediate neighborhood of the iodized oil droplets. Finally and most importantly, we believe, there is more or less complete degeneration in the lateral portion of the nucleus medialis dorsalis of the thalamus. In the early months after lobotomy this nucleus is the seat of considerable gliosis, but with the passage of months atrophy occurs with disappearance of both glia and ganglion cells.

Intelligence

Intelligence has long been considered a function of the frontal lobes. However, the tests employed have not been well adapted to the confirmation of this opinion. In fact, in our own studies, as well as in many others reported, the results in this particular situation have been inconclusive. Not infrequently we have observed almost identical scores with a battery of tests made before operation and as early as two weeks afterwards. Sometimes, the scores are even improved, presumably because of the better attention given by the patient to the problem at hand. We have probably been more fortunate than most investigators in our material, having now a fairly large series of intelligent young individuals coming to operation because of crippling neurotic obsessions, but whose contact with the world of reality has been sufficient to keep them out of institutions. These individuals are able to cooperate to a satisfactory degree in the test situation. Whether the tests comprise the regulation Stanford revision, the Kohs blocks, the cube construction, paper form board, substitution, color naming, or delayed choice tests, the results have been more or less in accord. Similar experiences are reported by Kisker, Hutton, and more recently in detail by Porteus. These findings are also in agreement with the general observations following frontal lobectomy as studied by Brickner, Nichols and Hunt, Wies, Hebb and Penfield, and Ackerly, although Rylander, in unilateral lobectomy cases, reached the conclusion that there was slight reduction in I.Q. Porteus has employed the maze in testing his patients' intelligence and finds a much greater reduction in capacity than is revealed by the Binet Simon test.

The more closely one analyzes the tests employed, however, the more certain it becomes that the tests in which the patients do badly are those in which other factors than native intelligence enter the picture. This was particularly demonstrated, in our opinion, in two cases of bilateral frontal lobectomy studied in a highly artificial situation by Gantt. We believe that the more constricted the test situation is, the more one has to contend with emotional reactions on the part of the patient, and hence the more erratic are the scores. Just as a patient's performance in a given test may reveal to the examiner far more than appears upon paper in regard to the reactions of the patient, so the test as administered brings into play more facets of the patient's personality. About all we can say, on the basis of studies of intelligence, is that it is not permanently harmed by the operation on the frontal lobes. Naturally, there is a period in the immediate postoperative phase when the intelligence cannot be adequately applied to the task in hand, and this period is lengthened in those cases in which extensive operation is performed.

As measured in terms of working ability, the intelligence necessary to operate a switchboard, a typewriter or an automobile recovers within a few days or weeks. Bookkeeping and cooking require greater attention and a sense of timing that are slower to recover. Learning new skills, or even entering upon more complicated situations, such as the practice of law or engineering or management, requires still further time, and, unquestionably, there is permanent lowering of effectiveness in many of the individuals operated upon. Whether this is purely a matter of intelligence, however, is debatable, unless the term "intelligence" is broadened to include planning capacity and enterprise, the setting of a goal, and planning of a course to attain it, and the adaptability to the occupation. This seems to be an unwarranted extension of the term "intelligence," since so many other factors, particularly emotional ones, enter into the total behavior pattern.

Allied to intelligence tests are a number of personality profile studies that are at once more vague and also more revealing of the alterations that have occurred in the patient as the result of operation. This is a field that is worthy of further development. At present, the Rorschach test occupies front rank in current interest in the psychological field. In forty cases tested for us by Dr. Thelma Hunt before and two weeks after operation, the conclusions were reached that quantitatively there was relatively little difference. Qualitatively, the test showed that there was less perseveration, less self-reference, less concern and self-criticism, less emotional tension, and that directed outwards, and finally, fewer indications of neurotic trends. Kisker has verified these findings. There is a tendency toward improvement in response with ensuing months in those patients that make satisfactory progress. Surprisingly absent are most of the "signs" of organic brain disease that have been emphasized by Piotrowski.

In a number of our cases, the Kent-Rosanoff and the Bernreuter tests have been applied with results corresponding fairly closely with those of the Rorschach test. It was surprising to observe how great a change took place within two weeks in the patient's opinions concerning himself. We concluded that this resulted from different attitudes and feelings toward his own behavior. There were indicated definite changes toward extraversion, self-confidence and sociability, aspects of the personality that had been estimated at a very low level by our psychotic and neurotic patients previous to operation, but changes which were generally borne out by their behavior in the ensuing months and years.

SOCIAL BEHAVIOR

Bianchi postulated the frontal lobes as being responsible for "social sense," and, in his monkeys, he described many traits that he considered were indications of a loss of this characteristic. Social sense is unsatisfactorily vague as far as definition is concerned, but, nevertheless, we have accumulated numerous examples of lack of tact and diplomacy, of arrogance, self-assertion, of childlike enthusiasm and lofty intransigeance, alternating with excessive politeness, humility, modesty and gratitude. On the other hand, gross boisterousness, silly euphoria, indecency, profanity, and knavery are rare. One looks in vain for evidences of altruism, of service to the community, of creative expression in writing, painting, and composing. With prominent exceptions, the people whose frontal lobes have been partially inactivated are very direct, practical, ingenuous and unselfconscious in their dealings with other people. They respond quickly, vividly and directly to the stimulus of the moment, take their cue from their interlocutor. They make enthusiastic companions, and they cannot be insulted. They are ready with their laughter, and quick at repartee, usually of a rather superficial variety, but without rancor. They flare up easily, become petulant on slight provocation, but they do not hold grudges. They take orders gracefully at times, but with outspoken resentment at others. They have only moderate control of their tempers, but the explosions, when they occur, are evanescent, and do not leave them shaken. They appreciate the significance of their acts afterwards, but either do not do so before, or they do not have sufficient self-control to arrest their impulses of the moment. There is a question whether they really feel deeply when they have been in the wrong, and, although they can apologize readily, they are apt to make light of the occurrence and to respond in the same way on the next similar occasion. They can learn by experience, but it is a difficult process for them, time-consuming and often not worth the trouble. If these people would count ten before they strike, it would be much to their credit, but they either cannot foresee the effect that their actions will have upon others, or they just don't care. Amid strange surroundings and with people they do not know well, these patients are apt to be on their good behavior, giving no offense, and conducting themselves with restraint and dignity, whereas, when they are in familiar surroundings and know that they will not be criticized, they indulge in their care-free behavior and give in to their puerile impulses.

It would be unfair to these people to describe them as wholly and completely devoid of social sense. When the convalescent period has passed, the majority of them can return to their place in society. They make earnest, reliable, sober workers, diligent, hard-working, varying as much in their individual responses to the outside world as do normal persons. Indeed, few of them would be considered abnormal. It is only when they are contrasted with their former selves that the differences appear. We are not intending to present the picture of lobotomized patients as if these people were outstandingly abnormal, since that is not the case. Indeed, it is chiefly because we are interested in the functions of the frontal lobes that we present the peculiarities of these people and emphasize the deviations, even selecting unsatisfactory cases, in order better to present the picture of individuals whose frontal lobes have been partially inactivated.

For example, more than half of the 240 patients treated by this method are sustaining themselves in a social environment, earning a living, keeping house, or doing part time work outside the home. Many more are already in advanced years, and since their families have closed ranks, these people no longer have responsibilities; they have retired, and they live in harmony with their relatives. There are some people of advanced years who would be able to live at home, were there some relatives sufficiently interested to make homes for them. As a matter of fact, only about 10 per cent of the patients who have been subjected to prefrontal lobotomy are confined in institutions. Some of those on the outside are living very restricted lives, more or less household invalids or even household pets, but nevertheless sufficiently integrated to care for themselves in the matter of the intimate details of daily life. At the present time, 90% of our patients are established in the community. It would seem therefore, that the human individual can function quite adequately in a social environment with a relatively limited portion of his frontal lobes. The adaptation possible in a given individual would appear to be conditioned to a considerable extent by the remaining quantity, since operations behind the coronal suture greatly reduce the ability to earn a living or keep house. On the other hand, in a sick individual, the possession of a rather small amount of frontal lobe tissue in front of the coronal suture may be associated with the presence of such functional disorders of behavior that the adaptation is even worse and institutional treatment is necessary. The 10% of patients necessarily confined in institutions represent for the most part inadequate operations or cases in which emotional deterioration previous to operation has been so great as to permit no adequate reintegration of the personality in the domestic environment. Only rarely is a patient necessarily confined because of erratic behavior following operation, as, for example, a lady who cannot say no to an auctioneer or an oil stock salesman.

The types of occupation that patients can exercise following prefrontal lobotomy are quite varied. Few of them are very high. We can point to no artists, writers, doctors, leaders of men or soldiers. One lawyer is working for the Government after burning himself out in private employ and suffering three years of complete disability; another is adjusting badly after giving up his governmental position to enter the army, only to get repeated courts martial for drunkenness and a Section 2 discharge; a third is working in a war industry. A dentist is institutionalized for alcoholism; a veterinarian, however, is carrying on successfully in a modest way. A draughtsman is making a superb adjustment, has patented a machine for grinding elliptical bearings, and is making more money than he ever did before; another is doing adequate work in map-making. Two men are engaged in managing their own small companies. One woman is teaching mathematics in a university. One man is at present teaching the mathematical basis of radio in the Signal Corps, and another is radio operator on a merchant ship. Salesmen, clerks, steel-workers, mechanics, foremen, janitors and so on are other representative occupations. Actually, women far outnumber men in our surgical cases, so that, in the field of sales work and office work, there are many examples, but none of these women has risen to a supervisory capacity or any other considerable position of responsibility. A former bacteriology technician is content as a salesgirl; two interior decorators are no longer functioning in this field; and a number of women have resigned or retired from positions which they held until shortly before operation. On the other hand, a number of women are now employed who never previously held jobs. Indeed, in several instances, these patients seem to be able to handle the social situations called for in office work with better ability than that with which they previously managed their domestic situations. Housekeeping would seem to be an occupation requiring a not inconsiderable degree of foresight and self-restraint, and these qualities are always affected in some degree following prefrontal lobotomy. It is to the credit of many women, therefore, that they are able to return to their exacting tasks; and we point with considerable pride to a lady who, since her operation, has married again and has borne two healthy children.

It may thus be seen that prefrontal lobotomy does not necessarily mean that a person who is disabled by one type of malfunction of the nervous system will merely exchange this disability for another that may be just as serious but more easy to tolerate. Prefrontal lobotomy brings about certain changes in the orientation of the sick individual that gives him or her an opportunity for cultivating certain capabilities that are of value to society. Even if the higher creative functions are not so much in evidence in the patient who has been operated upon, nevertheless, the functions that remain are consistent with active, pleasurable and productive existence in the highly competitive civilization of today. And yet it is not quite as bad as Ody suggested some years ago: "Society can accommodate itself to the most humble laborer, but it justifiably distrusts the mad thinker." For the most part, the patients have been reasonably capable, undistinguished, uncomplaining citizens, who continue to function in their environment with a minimum of strain. As Hutton states: "They are quite capable of taking thought for the morrow when the occasion requires, but they are content to let the morrow take care of itself."

THE FRONTAL LOBES ADD SOMETHING

The frontal lobes add something. It is the purpose of this investigation to determine as succinctly as possible what more could be accomplished by these patients if their frontal lobes had not been inactivated, with the proviso that the frontal lobes were functioning harmoniously. Later on, we shall study the harmful effects of perversion of frontal lobe function and its devastating effect upon the personality as a whole.

Substantially, the frontal lobes, in our opinion, add foresight and insight.

The individual with intact frontal lobes is able to construct a mental picture of what will occur in response to a series of activities that he is to initiate in the future. This requires imagination, which is one of the highest forms of cerebral activity. When confronted with a situation, the healthy individual gathers all the information available through his various special senses and calls upon his storehouse of knowledge concerning similar situations in the past, studying the situation as a whole as well as in its various particulars. Here is where the synthesizing activity of the frontal lobes is called upon as was pointed out so ably by Brickner. He assumes an attitude toward the total situation (Goldstein), which is largely conditioned by his emotional status at the time. Then, as he contemplates the problem, he focuses his attention first upon one aspect and then upon another, projecting himself in each instance into the future and assessing the result of each alternative that presents itself to him. He examines contingencies and probabilities, concentrating upon the relevant and excluding the irrelevant, the unlikely or the impossible. When he has finally satisfied himself that the problem is solved, he then presents to himself the particular steps to be taken in logical sequence in order to bring to fruition the plan that he has adopted. In other words, it is as though the frontal lobes were the focal point through which all the various "beams" of inner light come from the sources in past experience, are gathered together, and are then projected upon the screen of his life in the near or distant future. Schilder expressed this thought quite well: "A normal person has a plan; he lives in the schematic anticipation of what he expects. He is satisfied before he has reached his goal through trial and error, and when he compares the schematic anticipation with the actual result of his thinking processes. A patient with a lesion of the frontal lobe is much more easily satisfied than a normal person. . . . The patient seems to conclude his thinking processes more quickly than a normal person; he does not take time to 'finish up' his thought. In other words, the frontal lobe is necessary for the consummation of thinking."

Foresight is badly impaired for a period after operation. This was notably revealed in a restless and exuberant individual who made a lot of trouble at the hospital. He picked the paint off the armchair and the plaster from the walls; he pulled the screens out of the windows and slapped the nurses on the behind, smirking and making objectionable remarks. Shortly before his discharge, he fixed his attention upon a little red box outside his room and saw in large letters the word: PULL. If he had stopped to examine the box further he would have read the whole direction; "In case of fire PULL." Anyhow, he set off the fire alarm, and, when the nurses, attendants, and others came running, he was apologetic. We endeavored to ascertain what prompted him to pull the handle and finally he said: "Now that I've done it, I see it was not the thing to do, but beforehand I couldn't say whether it would be all right or not." As an example of deficient foresight, this could hardly be improved upon. The later history of this individual is of interest in demonstrating the return of a satisfactory degree of foresight. He had a difficult time of adjustment for many months following operation, finally culminating in a trip to Reno and a divorce. However, on the trip back East with his paramour, when she suggested to him that they get married and cut off the wife's allowance, he immediately rebelled, discarded the woman and returned to his wife. This was five years ago and the man is managing his own business and living at home more harmoniously than he ever did previously.

The return of foresight is relatively adequate in most individuals. There is a difference between the impersonal and the personal aspects of behavior, however, that is worthy of mention. Occupational adjustment demands greater capacity for foresight in the impersonal than in the personal sense. It is particularly in the matter of personal and interpersonal adjustments that foresight is impaired in patients who have undergone prefrontal lobotomy. A person may be able to formulate and carry out a plan satisfactorily, involving a considerable number of variables, whether it is in card games or business, cooking or law. Not infrequently, the plans made are quite adequate and the execution flawless. The outlook is practical and the approach quite matter-of-fact. While decisions are on the hasty side, they are quite apt to be correct. On the personal side, however, the decisions are apt to be over-hasty. The patient is as if unguarded, or insufficiently protected from saying and doing things that are undiplomatic, hasty, careless or tactless. There is a certain ingenuousness about personal contacts. The frontal lobes, therefore, would seem to be the mechanism whereby an individual is enabled to restrain himself in a personal situation, studying the situation and the people involved, with a delicate sense of feeling for those individuals and an almost unconscious effort to avoid offending. The individual equipped with harmoniously functioning frontal lobes is able to pursue a devious and diplomatic course, avoiding the pitfalls of social intercourse and maintaining his dignity. He is able to postpone the pleasures of eventual success, to dissemble his feelings, to play a waiting game, to simulate affability where distrust exists, to detect motives beneath the other individual's bland exterior and to avoid giving himself away.

In the more purely intellectual fields, the individual with harmoniously functioning frontal lobes is able to foresee what the product he is working on will eventually be like. He can construct things in his imagination, and then get them expressed in the medium he has chosen, whether it is on the typewriter, the piano, the canvas, the lathe, or the cookstove. Creative work of any considerable complexity is a closed book for most patients with prefrontal lobotomy, although there are a few shining examples of superior performance. Creative work demands foresight, but it also demands concentration, contemplation, a certain subjectivity, an abstract attitude; all difficult for the operated patient. Foresight must be backed up by energy in order to effect consummation, and the energy required for creative effort is of a high order. Presumably, this emotional component that implements the ideas is derived from the native energy of the personality expressed through the emotional mechanisms apparently residing in the thalamus. Time and again, patients have told us of their plans for the future, well-constructed plans that had a good chance of succeeding. However, the patients admitted that they procrastinated. As one lady said: "Every day I wake up and I think of all the things I'm going to do today, and then I put them off until tomorrow, and somehow tomorrow never comes." The frontal lobes supply something of the anticipated results, but when deprived of their thalamic connections they are no longer able to drive the individual forward in the pursuit of perfection. Patients whose frontal lobes have been inactivated are a little too satisfied, a little too content with things as they are, and they lack the drive to pursue a persistent yet flexible course toward a distant goal. They may have plenty of energy, but it is apt to be frittered away in the distractions of the moment, or to be concentrated upon some particular task of no great importance. The patient may be engaged upon a large project, but he becomes lost in the completion of the minor aspects of the situation. With intact frontal lobes, the patient keeps his goal in mind and refuses to allow himself to be distracted by details. His foresight, with the emotional energy to propel it forward, guides him into the channels that will be the most productive of eventual results.

The frontal lobes furnish the individual with a mental picture of himself as he is from moment to moment. They are not necessary so much for the evaluation of past behavior. A patient with inactivated frontal lobes may realize perfectly well that he has said or done the wrong thing, and may be able to give perfectly plausible reasons for such erratic, undignified or tactless behavior. Usually, however, the reasons are apt to be inadequate, and what it amounts to is that the patient has acted upon impulse and without the mature deliberation that comes with proper foresight. However, there is something more than mere foresight concerned in this type of activity, if we read the reactions of these patients correctly. There is, quite obviously, a forgetfulness of the self in that particular situation. For instance, a patient will pick up some chance remark, some error in grammar, a spot on the hostess's tablecloth, something that sets his imagination to work. He focuses his attention upon the incident, talks about it, magnifies its importance, worries it, comes back to it again and again, and insists upon discussing it, while his wife is trying to distract his attention from it by pointing out how undignified it is to act thus, and while the people around him are more embarrassed by his wife's distress than they are over the incident itself. Meanwhile, the patient goes on in supreme indifference to the social situation that he is creating. He is an "enfant terrible" in the original sense.

This lack of preoccupation with the self can lead to all sorts of amusing slips from dignity. We recall a middle-aged lady of rather formidable proportions who bullied her second husband, a mild-mannered little man, to the point where her friends protested, although he was able to stand a lot of it. "Why shouldn't I have my way?" she asked, "I've always had my way, and I don't expect to stop now." On another occasion, we observed her standing on a street corner under a car stop sign hammering vigorously on the door of a bus that was not stopping for passengers, and gesticulating at the driver with such enthusiasm that the passengers craned their necks and laughed. The spectacle was made the funnier by virtue of the husband who was trying ineffectually and rather pathetically to call his wife's attention to the fact that this was a car stop and not a bus stop.

Sometimes, the lack of self-consciousness permits the operated patient to go about negligently clad, particularly in the home environment. Housecoats and bare feet are occasionally reported, and in some instances both men and women have made rather a joke of the fact that they have to visit the bathroom quite frequently. Criticism produces little in the way of improvement in behavior, since these people are unaffected to a surprising degree by the opinions of members of the family and friends. One does not have to be guarded in criticizing the errors in social behavior of these individuals. They do not have the same capacity to feel hurt or insulted that they had before operation. In fact, the usual mildly couched suggestions that the slip is showing or that the shoestring is trailing is hardly sufficient to cause these patients to pause in their conversation. When it is considered that women, in particular, are apt to gain weight prodigiously after operation and to spread the seams of their clothes, direct orders to sew up the seam or put on a different dress are usually taken in good part and are then acted upon.

The frontal lobes give the individual an image of himself as he is appearing from moment to moment. He is able to observe his companions at the same time that he is presenting his proposition, whatever it is, watching for signs of approval or disapproval, leading the conversation around the way he wants it to go without giving offense or seeming obtrusive. He is able to formulate to himself the best method of gaining the end he seeks by noting the effect that his words, actions and manners in general are having upon those he is talking with. He is able to dissemble his feelings, and furthermore, is able to inject himself into the situation of the other individual, hearing his side of the argument, lending attention and sympathy, offering a suggestion or two without necessarily becoming autobiographical about it. He can listen patiently, knowing that his turn for speaking will come later, and preparing his words in advance in a manner suitable to the situation.

It is almost impossible to call upon a person who has undergone operation on the frontal lobes for advice on any important matter. His reactions to situations are direct, hasty, and dependent upon his emotional set at the moment, without the reserve and dignity that comes from restraining the self in more mature deliberation. There is something childlike in the directness and ingenuousness of people after lobotomy. The frontal lobes evidently make for delay in the enunciation of opinions, for reserve in the statement of attitudes, and, at the same time, for a deeper appreciation of what goes on within the consciousness of the individual.

It is hardly fair to the patient who has undergone prefrontal lobotomy to say that he is entirely lacking in insight, since he is apt to be well aware of the effect he is having upon the people around him. On the other hand, it is rare to have a patient go at all deeply into his introspections. He is no longer particularly interested. The process of turning the thoughts inward is more difficult and laborious than it is in the case of the normal individual, and apparently is seldom considered to be worth the trouble. One may say, therefore, that the frontal lobes are concerned in furnishing to the individual a certain awareness of himself more or less heavily charged with emotional feeling tone, and that this awareness enables him to meet and converse with his fellows with a sufficient display of dignity, reserve, interest, patience, altruism, sympathy and helpfulness. Furthermore, the frontal lobes are apparently concerned in the higher social attributes of self-sacrifice, idealism, and exaltation in the contemplation of the infinite. People whose frontal lobes have been inactivated retain the manners of their social upbringing and many of the outward forms of social intercourse, but they are direct, practical and uninspired.

We have pointed out that consciousness of the self applies not only to the social situation, where the individual is a unit of society, but also to the bodily situation, where the individual is a collection of organs. The consciousness of the self in the latter field is reduced equally or even more so than in the former. After prefrontal lobotomy, patients lose their awareness of their hearts and stomachs and colons. Part of the gain in weight that is often observed may be due to the fact that these people do not realize when their stomachs are full. One of our patients developed a vesical calculus after operation, and it was only when his wife noticed bloody urine that he returned to the hospital for cystotomy.

We had occasion recently to observe a young woman whose vasomotor system had always been unstable. In the presence of strangers we demonstrated her as an example of successful occupational adaptation following prefrontal lobotomy. We then called attention to the fact that she was blushing and made some rather pointed remarks upon this phenomenon, asking her if she felt uncomfortable. She denied that she was either embarrassed or uncomfortable. Meanwhile, the blushing spread not only over the face but over the upper part of the body and arms, the conjunctiva became flushed, urticaria broke out, the patient was unable to eat lunch and shortly thereafter vomited, became faint and had to lie down for two hours until the reaction passed away. Throughout this whole reaction she denied that she was suffering either humiliation or discomfort. Only the next day she stated that she didn't want to be demonstrated again that way. It had been too strenuous.

Our observations upon visceral distress in relation to the frontal lobes have been rather impressionistic, but we have been successful in almost every case in relieving such distress as occurs in hypochondriacal individuals, in the psychoneuroses and in one case of severe conversion hysteria. Furthermore, in conditions in which there are severe and painful arthritic, muscular or nervous disorders, we have observed disappearance of the distress, even though the pain and other disability might continue. In the case of hysteria, for instance, there were decided arthritic changes, not only in the spine but also in the lower limbs, and when the patient was first pulled out of bed after two years of complete invalidism, we could hear the crepitus across the room. The patient walked of her own accord, even after she sprained, in turn, each of her weakened ankles. Now, more than seven years after lobotomy, she still has pain in the back and limbs, but it does not disable her; in fact, unless she is asked about it, she never mentions it. In another case, there were definite indications of injury to the cauda equina with severe vesical tenesmus and complete disability. Prefrontal lobotomy relieved the pain and distress after half a dozen local operations, including two explorations of the cauda equina had failed. Some patients, after undergoing lobotomy have died of other diseases, one in particular of carcinoma of the colon. We were informed that she bore the pain with "amazing fortitude."

From these observations, we are inclined to draw the conclusion that the frontal lobes are of importance to the human economy in interpreting pain. Patients react to painful stimuli in just the same way following lobotomy as they did before, but they no longer anticipate pain to the same degree. We have sometimes made gestures toward a painful area as though to injure it, but without observing any defense reaction unless the part was actually contacted. The person with intact frontal lobes is made aware of sensations arising from his body and of their implications in terms of pain, disability and future consequences. Such sensations arising from the body suffice to guide him, for instance, in the cessation of eating before abdominal distention becomes noticeable. They terminate his physical activity before he becomes exhausted. They cause him to seek medical advice when the explanation of discomfort is inadequate. They cause him to seek the cause and relief of distress, and, under normal conditions, to bear with equanimity and patience the disability. The frontal lobes are not necessary, apparently, for protection against gross threats to safety. People who have undergone prefrontal lobotomy do not cut or burn themselves. They do not drink poisons or overdose themselves with medicines. Sometimes, they are injudicious in the use of alcohol, being apparently unaware of the effects that the liquor is having upon them until they have become intoxicated, and they are more sensitive to the effects of alcohol than before operation. It is in the more delicate adjustments to life with the body that the frontal lobes apparently carry out their protective functions.

PERVERTED ACTION AND THE FRONTAL LOBES

We have discussed in the preceding sections the part played by the intact frontal lobes in the normal individual, how these parts of the brain contribute to the fuller, deeper, richer life of man, how they aid him in conforming with the rules of social intercourse, how they make for progress in intellectual and spiritual fields, how they help him cultivate a decent, restrained, sympathetic helpful, cooperative and altruistic attitude in the world of his fellows. We have emphasized the value of the frontal lobes to the individual in maturing the whole personality, in the cultivation of intellectual interests, in the urge for perfection in the various accomplishments of the arts and sciences. We can quite agree with Tilney that they are the instigators of progress, with Bianchi, that they underlie the function of social sense. How, then, can these frontal lobes conduct the individual to the brink of psychosis and suicide? How can they reduce him to the filthy, resistive, combative, assaultive, violent patient, utterly unable to care for himself and to observe the decencies of social existence? How can they permit him to spend decades on the back wards of our state hospitals, all but forgotten by relatives, while the stream of life goes on outside the institution? What purpose is served by the vaunted frontal lobes, if they reduce man to this degraded state, with behavior worse than that of the caged animals at the zoo? The frontal lobes? Pathological excrescences!

Let us examine the functions of the frontal lobes in their relation to psychopathology.

We have gone over in rather detailed fashion our concepts of function of the frontal lobes. Foresight and insight are the two functions of prime significance for the individual in a social environment. What, therefore, do foresight and insight have to do with the development of psychosis? How do they contribute to the demoralization that is characteristic of the early phase of psychosis?

We believe that the frontal lobes are ordinarily the seat of an extremely varied functional activity. We have shown how they are capable of directing the attention of an individual to the minute ramifications of a problem, how they bring to the individual a capacity to respond to an abstract idea. Given an individual who is faced with an important decision upon which future projects depend, the thought and contemplation expended upon the proposition may be sufficient to fatigue the individual so that his powers of envisaging the whole problem become increasingly impaired. In a state of fatigue the capacity to synthesize is reduced and the individual is obliged to undertake the consideration of each element in the proposition, in turn, rather than many aspects simultaneously. At the same time, his capacity for achieving satisfaction with his solution is reduced, and he spends more and more time on a fruitless quest for the solution. With the lack of solution of the problem comes, at first, a vague and ill-defined discomfort, an emotional reaction to a problem that is not being solved as adequately as he thinks should be possible. The emotional reaction to the unsolved problem not only makes the solution more distant, but also tends to focus the attention of the individual upon himself, and he searches his mind for reasons why he cannot solve the problem that, under ordinary circumstances, should give him no particular difficulty. With the contemplation of the problem, the emotional charge becomes deepened, and the concrete attitude turns more and more toward the abstract. Instead of asking himself, "What shall I do next?" which has an answer, he says: "What shall I do?" And this question has no answer.

The frontal lobes, which give us the power to solve problems, also give us the power to erect new problems on the basis of the old that stretch too far into the abstract to permit of solution. The reaction to failure is with emotion that further upsets the normal balance of mental activity, leaves a decided feeling of frustration and an emotional surge that makes even tackling the same problem a difficult and unwelcome one. Instead of finishing up his thinking, the individual with intact, but (shall we say?) fatigued, frontal lobes continues his thinking beyond the useful period into the useless and harmful period in which blocking and indecision are the eventual outcome. A normal individual has a problem to solve. It may be easy, in which case a solution is quickly adopted and acted upon. It may be difficult, in which case prolonged thought may be necessary, but the solution is acted upon, and, if successful, there is the gratification that naturally follows. If the solution is unsuccessful, there is the usual disgust, with a determination to learn the lesson and do better next time. But, worse than failure, is the lack of any solution, for this leaves the problem where it was in the beginning with an addition of the knowledge of failure, and possibly the result that no answer can be expected. The normal individual may recognize his inability to solve the problem, but the individual who is uncomfortably perfectionistic is not satisfied with this relinquishment and he tries again and again, with equally poor results and yet with an inability to give up contemplation of the problem. He remains hung up, *accroché,* as Janet says, able neither to solve the problem nor to abandon it, and the conflict and frustration increase the load of futility that he is already carrying.

The normal individual scans the situation with a certain philosophical resignation and admits that he isn't as clever as he thought he was. The individual who is becoming sick is not satisfied with a simple answer like this, but seeks within his own consciousness the answer to his failure. The problem which at first was only intriguing has become pervasive, something threatening, something malignant. He wonders why he cannot summon his mental powers to solve it and, in the hours of darkness, cannot relinquish the problem for the sleep that might clarify the situation. He spends his hours going over and over the situation making and unmaking decisions until he falls into the fitful sleep of exhaustion, with vivid dreams that do not bring the answer.

With continued failure to produce an answer, the sickening individual begins to examine himself (frontal lobes!) and to explore his mental processes, endeavoring to ascertain what is wrong with himself, meanwhile sinking more and more into a mire of self-directed thinking, from which he is roused with difficulty by his relatives. He shuts himself away from his friends and his family, gives himself up to the contemplation of himself, with steadily increasing emotional surges that lead slowly or rapidly as the case may be to a feeling of utter futility and frustration, with death as the only apparent solution.

The selective activity of the frontal lobes may thus be perverted and carry the individual through the throes of self-contemplation and self-transplantation into the future until self-destruction seems altogether logical.

As another example of perverted activity of the frontal lobes, we may cite a case in our experience in which a mild cutaneous eruption, occurring shortly after an emotional experience, was the precipitating factor in a syphilophobia that lasted for twelve years and was only relieved by a rather extensive prefrontal lobotomy. The case is given in some detail in our monograph as an example of the manner in which a simple incident may be transformed by emotion into a disabling contemplation of the self and the future with all its horrendous potentialities.

Actually, it is our belief that the ideas developed in a neurosis or a psychosis are unimportant. Anybody may have a few crazy ideas. Which of us, when confronted with a local pain or swelling or eruption, does not cast about for an explanation? If the explanation is readily determined, the proper treatment is undertaken and the episode is all but forgotten. If the explanation is not immediately forthcoming, the individual, by means of his frontal lobes, goes back into his past for the possibilities, and forward into the future for the eventualities. Among our patients, and particularly among ourselves who already know what can happen, the most threatening prospects are the first envisaged. Tuberculosis, syphilis, cancer. These ideas are normal, they are natural, they are the product of a normally functioning pair of frontal lobes. The point is that the ideas developed in connection with the local disturbance are not fixed. They come and go, are changed around, and the less likely are discarded. When the solution comes and the future seems relatively clear, it is accepted, even though it may be an unfavorable one, and the attention is diverted into more profitable fields.

The difference between this reaction and the pathologic one resides in the fixation of the ideas. Egas Moniz particularly called attention to the fixation of ideas which he considered were in relation to relatively fixed constellations of neuron patterns, and which he believed could be broken up by prefrontal leucotomy. Taken in this light, then, the fixation of the ideas becomes dependent upon the interaction of the frontal lobes with the thalamus, the emotional component having the function, as it were, of fixing the idea. This fixation by emotion may be compared with the production of a photographic image. Following exposure of the sensitized film in the camera, there occurs a physical change in the emulsion which is not apparent until a reducing agent is applied. As the developer acts, the image is brought out, but if the developer continues to act the image fades back into obscurity. However, if, at the proper moment, a fixative is applied, the image retains its brilliance and neither light nor reducing agent will then be effective in changing it. Ideas are harmful not because of their strangeness or queerness, because anybody may have a few strange and queer ideas. It is the *fixation* that renders them harmful; therefore, *fixation* becomes the criterion of psychosis.

Once fixation of ideas has occurred, the stage is set for further elaboration. This may be either along the lines of somatic preoccupations (hypochondriasis), intellectual preoccupations (obsessive states), or social preoccupations (paranoid reactions), or similar conditions. The full elucidation of the psychopathology of mental disorder is beyond the scope of our presentation. We would emphasize, however, the interplay of the intellectual and emotional components of the mental processes in terms of neural activity. The emotional reactions impose upon the behavior of the sick individual an ever increasing burden of abnormal ideation, until, with emotional deterioration setting in, there is gradual and progressive obliteration of all semblance of normal integration of frontal lobe activity. When this occurs the whole personality of the patient becomes degraded and only life remains.

SUMMARY

The frontal lobes have many functions, but those which we have been able to demonstrate most conclusively are concerned with two very complex and personal functions, foresight and insight. When the frontal lobes are inactivated by an operation severing their connections with the thalamus, there develops a type of behavior that is quite adequate at the concrete social level. People with a satisfactory background who have not undergone too much emotional deterioration are able to maintain themselves quite adequately, and most of them can resume a constructive place in the community. They are lacking mostly in an emotionally charged attitude toward themselves and toward the future that is characteristic of the mature individual. While they are usually cheerful, friendly, unselfconscious, alert, willing and emotionally responsible in social situations, there is nevertheless a lack of adult dignity, of thoughtfulness, of sympathy, altruism and general helpfulness that we look for in an entirely adequate individual. Introspection is difficult and unprofitable for most of these people and they take life as it comes without too many regrets for the past or fears for the future. Only seldom does an individual step over the bounds of decency and reasonable self-restraint.

We are led to the conclusion from observation of a considerable number of people who have recovered following prefrontal lobotomy that the frontal lobes are capable of considerable reduction without impairing the social effectiveness of the individual.

On the other hand, we may argue that under certain circumstances, the anatomically intact frontal lobes may undergo a certain perversion of activity in response to forces that are widespread and for the most part well understood. As a result of increased emotional toning probably supplied by the thalamic connections, the frontal lobes become fixed in their responses. Thenceforth, stimuli of various sorts coming to the consciousness of the individual may be considered as channeled through the abnormally fixed constellations of neuron patterns in the frontal lobes, resulting in abnormally fixed behavioral reactions. That the fixation is determined and continued by the emotional component is indicated by the altered reaction observed following the severing of the thalamofrontal pathways. The following sequence is regularly observed after lobotomy: first, the ideas lose their emotional potency; then the pathologic ideas decrease and disappear; and, finally, the motor component in association with the ideas, that is: the compulsions, the mannerisms and the tics, tend slowly to die out. Once the emotion in connection with the ideas is dispelled, the ideas lose their significance to the individual and the psychosis dies of inanition. The resulting personality is not all that could be desired, but it is considerably more adequate than that which persisted in the demoralized state associated with the psychosis.

CONCLUSIONS

The frontal lobes are the noblest endowment of man, essential for progress up the stages toward perfection of society. They furnish the individual with the capacity to see himself and to see into the future. If he sees steadily and objectively all is well. If he can see only himself, and if his vision of the future is colored by fear, remorse, shame, guilt and more fear, and, above all, if his vision becomes fixed, then the frontal lobes can lead him to psychosis and suicide. Without the frontal lobes, there would be no functional psychoses.

DISCUSSION OF THE PAPER BY DOCTOR FREEMAN and DOCTOR WATTS

BY DR. S. BERNARD WORTIS

This paper is a most important contribution to our knowledge of the functions of the frontal lobes in man. Race after race of man has appeared on this earth, lasted but a short span of time, and then met disaster and extinction. Our modern race has persisted, because we have evidence that it differs in quality from its forerunners chiefly in its cerebral endowment. The frontal lobes have guaranteed modern advantages to man, brought him his spiritual understanding, his social attributes and his satisfactions from art and literature. Eventually, we all hope, world cooperation and recivilization will be the final achievement of the brain, especially the frontal areas. Tilney aptly spoke of the brain as the master organ of destiny, the cumulator of experience, the instigator of progress. These abilities of the human race are specially concerned with the frontal areas of the brain. We know, from all the evidence accumulated, that the frontal lobes are concerned with foresight, the capacity to project oneself in time and space, abstract thinking and social attributes with moral and ethical features, and insight with the capacity for self criticism. These superego functions, concerned with the interpersonal relations, have important relations with the frontal lobes.

May I remind you of one of the earliest patients with frontal lobe lesions:—one Phineas Gage, a well balanced, shrewd foreman of a construction gang who accidentally had a crowbar driven through his head, entering below his left malar bone and coming out between the frontal bones? He subsequently became "irreverent, profane, impatient of restraint, obstinate, capricious, vacillating." Later he had convulsions and died.

The frontal areas of the brain are large and represent large association areas. Intellectual function requires an immense number of association pathways, but joining specialized cortical areas. Normal, intelligent functioning, consists of an immense richness of conditioned responses. In frontal lesions, no special quality of behavior is lost, but there is a quantitative deficit.

Medical progress depends on much experimentation. Often the cure of a disease seems to have little logical relationship to the cause. For example, we give patients who have general paresis (syphilitic disease of the brain) another disease, malaria, to cure them of the paresis. Such therapy has little logical to recommend it. Why does giving a person a second disease cure an existing disease? In this light, it seems to me, we must evaluate frontal lobotomy. Is the surgeon justified in depriving a person of the most important part of his intellect in order to relieve him of emotional difficulties? We have heard from Doctors Freeman and Watts that such patients after operation, are often lazy, undiscriminating, and have impairment of insight. True, some lose their obsessive worries, but at the expense of impairment of the highest integrations of their conscience or superego functions. It therefore seems to me that other less drastic therapeutic methods should be used first. The procedure of frontal lobotomy is more appropriately used in old chronically ill patients—especially those with prolonged agitated depressions who have not responded to other, less drastic, reversible treatment.

Frontal lobe function has both phylogenetic and ontogenetic significance. The infant, with little association frontal lobe development, has poorly developed superego functions; he socializes poorly, is egocentric. The normal adult has good frontal lobe function with some capacity to project himself into the future. This makes for social progress. The senile person has arteriosclerotic changes in the brain; he again becomes the asocial, egocentric individual who looks backward, not forward. So, you see, there is a development history of the frontal association areas.

The more important contribution of this work is the information it has given us concerning the functional significance of the frontal areas. I am certain I express the opinion of this group when I tell Doctors Freeman and Watts, we have enjoyed their most stimulating scientific contribution to basic neurophysiology.

SECTION OF ANTHROPOLOGY

May 22, 1944

Doctor James W. Woodard, Temple University, Philadelphia, and Queens College, Flushing, N. Y.: *The Role of Fictions in Cultural Organization.*

The viewpoints presented in this analysis have slowly emerged over twenty years of teaching and protracted mulling over of the areas concerned. Into it have gone many items from sociology, anthropology, and psychology; and endless filtering of one's own experience through his categories, with resulting modifications, supplementations, corrections, and occasional dearly-bought illuminations. For such a "synthetic distillation," descriptive material is necessarily used for its expositional value, rather than for proof. The validity of the analysis is testable against the reader's own experience and background, and the reader will be a qualified critic to the extent to which (and in proportion to the background, experience, and maturity with which) he has been carrying on a somewhat similar attempt to synthesize these chaotic materials. If there are main sources and derivations, they probably lie in the work of Sumner, Max Weber, Wm. James, Freud, Adler, Vaihinger, Durkheim, Marx, Malinowski, and Veblen, a motley array, somewhat recalcitrant to synthesis, but alike in their functional approach, and in cutting below the surface of things to show us things-as-they-are. The work of other writers, Cooley, G. H. Mead, Horney, Fromm, Pareto, Benedict, Waller, etc., would also have relevance to such a synthesis as is here attempted. The seriation—folkways, usages, conventions, mores, and institutions—is a needed improvement in precision over Sumner's vaguer, less discrete, and more inconsistently used folkways-mores-institutions seriation. The "as if" of Hans Vaihinger, the "life lie" of Adler, the pragmatism of Peirce, James, and Dewey, and the functional-processual approach of Freud, Marx, the functional anthropologists, and all the writers just named are reflected in the stressing of the importance of these principles in phenomena at the societal level of abstraction.

The structures and processes of society must be functionally approached if they are to be understood. Society carries its "as ifs," fictions, and "life lies," things that are not objectively true, but belief in which is vital for the retention of the given societal integration. Vital "beliefs" or "functional truths," one might call them as over against "objective truths." For, however much the pragmatic criterion must be set in the perspective of other criteria for the purpose of philosophy and science (and *eventually* for life itself for that matter), the naively carried on societal process, like that of the individual organism, does seem to carry itself along on pretty much of a pragmatic basis—in terms of what works (or seems to work) from the center of reference of the functioning unit concerned.

Thus, there is much control by illusion—by myths, legends, biases, rationalizations, fictions, and outright lies—which will not stand the scrutiny of objective truth. The full implications of this; the necessarily referential and relative character of meaning itself; etc.; these things would take us far beyond the spatial limits of the present paper. And we bespeak the reader's indulgence if we seem to touch too lightly or too superficially on some aspects of our study. Also, we should like to make clear that we are not discussing the role of fictions in all areas of societal phenomena—the myths, biases, and legends of social organization and the social process, for example—but only in the area of the cultural organization. Even here, we have further limited our analysis. For we shall not discuss the fictions of the inductive culture—the abstractive fictions and fictional constructs of science, with its light bullets, electrons, endopsychic censors, typologies, etc.—nor those of the appreciative-expressive culture—the "let's pretend" of all play, pageantry, drama, and fiction, the formalization of representational aesthetic motifs, etc. We shall limit our discussion to the fictions of the control culture itself. And we shall treat the control culture *as* culture. Thus repercussions relative to class structure, class struggle, war, revolution, the social process, etc., will also be slighted. We hope the reader will be aware of these failures of inclusion, but will remember that we are spatially limited and that many of them would be taken care of in a statement of our full analysis.

Almost all social and cultural structuration and processing derive from the paradoxical nature of group living, as such. On the one hand, the social "units" are wrapped up in their separate skins; they have their separate centers of reference for meaning and value; hence they pristinely and persistingly tend to go off at their separate behavioral tangents. Yet such intransigent units must somehow be conformed to at least that minimal uniformity of behavior and belief requisite for group living. For the possession of minimal ready-at-hand patterns of behavior and belief is an inexorable situational necessity in the nature of group living. This is true in order to (1) economy of time and effort, (2) survival in a crisis, (3) a framework of predictability for intra-group behavior, and (4) a framework of meaning and value for the development of those consistencies in habits of thought and action that make possible the integration of the individual personality and character within its societal milieu. These are universal situational imperatives. In terms of them all groups everywhere always have *an* order, a *status quo*, a *cultura qua*. *Which* order, whether polygyny, polyandry, monogamy, etc., is another matter. But no group can remain a group without *an* order of minimal uniformity of behavior and belief to which its pristinely tangential units conform.[1]

It is obvious that this whole spontaneity-conformity problem will alter depending upon (1) whether a given item is vitally *important* (or so considered) or relatively *unimportant* from the center of reference of the group; (2) whether the individuals show *acquiescence* or *resistance*, i.e., whether individual tendencies spontaneously approximate a consensus or whether they have to be in some way brought into conformity; and (3) whether culturally carried patterns are *provable* or *unprovable* to the individual mind as to the utility of the behavior or the truth of the involved beliefs. For only to the extent to which spontaneous consensus or rational proof are not available will recourse have to be had to disciplining, persuading, hoodwinking, or forcing. If the matter is of no importance to the group in terms of the four functions of uniform patterns mentioned above (i.e., economy, survival, predictability, etc.), individuals may be permitted to do as they please. Any cultural patterning in such an area will tend to occur spontaneously and without coercion or rationalization, the patterns being followed because of their common enjoyability and realizational appropriateness. This results in the *appreciative-expressive culture,* made up of the recreational forms, sociability forms, folk dances, folk art, folk music, pure art forms, schools of cookery, and the like, which are done for their own sakes on an end-in-themselves basis. Where there is objective proof available as to the utility of the behavior pattern or the truth of the involved beliefs, then there will slowly emerge an area of the culture characterized by experimentalism, skepticism, and rational means-ends procedures. This is the *inductive culture,* made up of the tools, devices, and machines, the supporting skills and techniques, the accompanying background of factual and theoretical knowledge which eventually make up the sciences, and the underlying methods of achieving certitude: logical demonstration, experimental proof, statistical and causal analysis, etc. But where matters are important to society, where the pristine individual tendencies are obstreperous and at variance, and where there is not (or not as yet) possibility of inductive proof bringing a rational consensus, then society must do as the individual does in a similar circumstance. It must raise its voice, become insistent, intolerant, and dogmatic, and through cajolery, emotional persuasion, moving pageantry or ritual, the pseudo-logic of rationalization, stigmatization, fictions, rewards, or force, produce the required conformity. There then results the *control culture,* made up of the prescriptions and taboos, the customs, traditions, and laws; more specifically, of the folkways, usages, conventions, mores, and institutions, together with their supporting fictions, ideologies, rituals, institutionalized roles, and so on.

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[1] There is nothing mystical in such functional concepts as situational imperatives, situational emergents, and the like. The cause-and-effect instrumentation of such concepts consists: (1) at the biological level, in fluctuations and mutations and their role in meeting the imperatives of survival; (2) at the physiological level, in fluctuations and mutations plus the physiological gradient, chemical balances, and other "organismic" modes of adaptation; (3) at the psychological level, in fluctuations, mutations, and physiological modes of adjustment, plus the alterations in conditioned response patterns and in gestalt formation through trial and error and covert trial and error (intelligenc[illegible] adjustive potent[illegible] the crystallizatio[illegible] there are failures and errors as well as successful trials, so that imperatives are not always met or fully met. But (1) the developed organism, established species, and enduring society has had to reflect them in the very character of their structures and processes. And (2) they are present factors in the understanding of failures, species-extinction, etc. Thus, situational imperatives are factors showing certain uniformities in their effect-*tendencies*. This is of the essence of scientific description. As such, they are scientifically abstractable and describable.

There is much to be said of these three parts of the culture and their relation to each other.[2] There are strains between them. Necessarily, the control culture dominates the entire culture at early points in cultural evolution. And it yields ground grudgingly as the inductive culture slowly undermines it, assimilates it, and alters its character, thereby winning release for itself and for the appreciative-expressive culture to carry out their own contained imperatives. In advanced cultures, quite divergent emotional-intellectual "climates" come to pervade these three divisions. This might be typified by the contrast between a Bohemian artist in his artistic sub-group, a research scientist in his scientific sub-group, and a bishop, humanitarian, or grand inquisitor in his moral-religious sub-group. They throw up their diverging immediate aims and developed, contained ideals. The inductive culture represents the reality principle. Utility (e.g., the provable utility of a steel knife over a bone knife is its immediate aim. Truth is its developed, contained ideal. The appreciate-expressive culture represents the pleasure principle. Enjoyment (as in a folk dance or a ball game) is its immediate aim. The integrative harmony of the fully creative and realizational, the beautiful, is its contained ideal. The control culture represents the moral principle. Conformity to the already given moral order is its immediate aim. But it holds a contained strain from tribal, authoritarian morals toward a welfare ethics. The social good is its contained ideal. Each division adopts the fiction that its own ideal really contains the other two, that it is the preeminent "way of life" for the group. Thus, there are strains, conflicts, and endless accommodative devices and resultant processings. But there are also strains toward synthesis.

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[2] See James W. Woodard, "A New Classification of Culture and a Restatement of the Culture Lag Theory," *Amer. Soc. Rev.,* 1 (1): 89–102.

This structure of culture parallels the structure of personality.[3] The moral-principled control culture and the moral-principled *super-ego* alike reflect the pristinely repressive, situational imperatives of group living, with its group center of reference for meaning and value. The pleasure-principled appreciative-expressive culture and the pleasure-principled *id* reflect the realizational strivings with their individual center of reference for meaning and value, while the inductive culture and the *ego* reflect the reality principle of impersonal reason —the impersonal and deracinated reference for meaning of the natural order of cause-effect reality. Of course, the overlappings and repercussions are far more complex than this simplification would indicate, for all of these factors are pervasive and permeate all areas both of culture and personality to some degree.

The points of interest to us in the present discussion are as follows: First, this is a functional classification of culture. The occurrence and characteristics of structures are understood in terms of their functions. This funtional approach we shall carry with us to the further analysis.[4] Second, we have seen the divergence of three centers of reference for meaning and value (the individual, the group, and the impersonal and deracinated descriptions of objective science) reflected in the threefold structuration both of culture and of the personality. Finally, it is evident that the strain between these divergent definitions of meaning and value will be at its greatest in the control culture. This portion of the culture owes its existence, structuration, and characteristics to the necessity to accommodatively reconcile that strain. We now turn our attention more specifically to the control culture.

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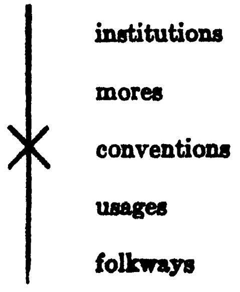
[3] See James W. Woodard, "The Relation of Personality Structure to the Structure of Culture," *Amer Soc. Rev.*, 3 (5).

[4] On the functional approach in the social sciences, see James W. Woodard, "Social Psychology" in the forthcoming volume, *Twentieth Century Sociology*, Georges Gurvitch, Ed.

It is obvious that not all prescriptions and taboos are equally insisted upon. In our own group, for example, it is rather heavily required that one not commit grand larceny or murder, rather lightly required that one eat his peas with a fork or tip his hat to a lady. This is obviously because so much depends, from the standpoint of other group members, on the former, so little on the latter. But the insistence also measures the resistance. For neither does it cost the individual much by way of discipline or self-sacrifice to eat his peas with a fork rather than a spoon; whereas to starve rather than steal, to go loveless rather than violate the chastity mores, or to curb the murderous impulses of rage and jealousy require a great deal of discipline and often considerable self sacrifice. The group will not have to insist if the individual acquiesces spontaneously, and the individual will acquiesce readily if the cultural pattern accords with pristine spontaneity anyway. Thus, somewhat like over-protestation and defense mechanisms in the individual personality, great insistence, intolerance of questioning the moral frame of reference, and elaborate supporting rationalizations and ritualizations measure not only the imputed importance of the pattern to the group, but also the extent and, to some degree, the natural-order legitimacy (from its own center of reference) of individual resistance.

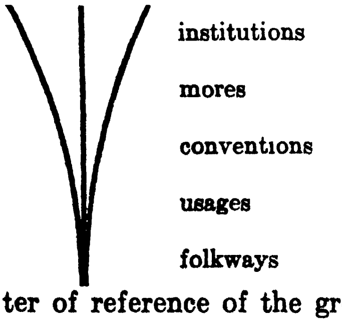
Just as, in economics, the intersection of the lines of supply and demand determine price, so here the intersection of the group's insistence and the individual's resistance determine where the particular culture pattern will be located on the scale of folkways, usages, conventions, mores, and institutions. A somewhat similar heuristic graph could be made use of by sociology. And, since a pattern may lie at any point in the important-unimportant scale or at any point in the acquiescence-resistance scale, it is evident that folkways, usages, conventions, etc., are not sharply discrete, but that they merge imperceptibly into each other. They are abstractive constructs arrived at in the course of functional analysis, not empirical types arrived at by statistical multimodalities of their scattering along the total range of behaviors. But, while they merge



imperceptibly into each other, at each point where we have added a new concept, significant new characteristics accrue. This we shall see.

At the *folkways* level, e.g., as we here more sharply delimit the term, there is no divergence between the three centers of reference for meaning and value already mentioned. We may take the culture pattern of the siesta in the tropics as our type-example. The group does not insist. The individual does not resist. A *functional* science, such as physiology, would stamp the behavior as functionally appropriate behavior for an organism such as man in an environment such as the tropics. Desire and duty do not clash with each other nor with truth. There is no resort to fictions, supernatural sanctions, illusion, bulldozing, or force by the group; and no hypocritical pretense or personality conflict on the part of the individual. Emotion, reason, and behavior fully buttress each other. The good, the true, and the beautiful are one. Man, at one with himself, is at one with his society and with his universe. The ideal-goal of ethics, psychiatry, and the social sciences (as functional sciences) is to reduce the totality of human-social living to this sort of contained harmonious integrity.

As we shall see, this is not really possible, though it could be approached as a limiting upper asymptote of human adjustment, so to speak. But the initial paradox contained in social living (of tangentially inclined entities with a skin-contained center of reference having to adjust to a social-cultural order with its own center of reference) makes it inevitable that, on more important matters requiring greater individual discipline or frustration, the three centers of reference diverge more and more. This could be represented graphically by a curvilinear Y plus a central column, each line of which represents one of the situationally diverging centers of reference for meaning and value. Let the central line represent "objective" meaning and value, as determined by a developed functional science. Let the left-hand curve represent meaning and value from the pristine center of reference of the individual; and the right-hand curve that from the center of reference of the group.[5] At the folkways level, the three lines



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[5] Like the neural-arc diagrams of psychology and neurology, this is necessarily vastly oversimplified; e.g., there are as many left-hand lines as there are individuals in the group; and they would have to be inter-threaded with their various sub-groups in turn, each of which (e.g., a sub-cultural area or level, a family, community, or interest group) has *its* center of reference also, which only partially corresponds with, and partially diverges from, that of its constituent members and that of the total society.

are one (redolent of the Trinity symbolism in Western culture!). But they diverge more and more as we go from folkways up the "ascending scale of self-consciousness and compulsiveness" to institutions. It becomes the business of the control culture to make the individual act "as if" only the group's line of reference had validity. This it may do by denying the essential validity of meaning and value as defined from the other lines of reference; and/or by admitting the situational disparities and/or then securing compliance by compensating psychic rewards, "spiritual" values, and the like and/or other modes of reward and punishment up to promises of eternal life, inquisitorial force, or the threat of hell fire.

We have taken the siesta as a type-example of the folkways. The reader can readily supply other examples, and from various cultures, such as the pattern of sitting on the front porch in the cool of the evening, as in the hill-billy South; of the social call accompanied by tea and gossip as in Scandinavia; of serving applesauce with roast duck, etc. Such folkways are as comfortable as old shoes. They are so appropriate that often they have had a syncretistic growth, being true "group habits." Members of a culture are sometimes scarcely aware that they follow them until, going to another group, they realize that there different folk "ways" are followed. The folkways thus merge imperceptibly into the appreciative-expressive and inductive cultures. The same realizational and rational appropriateness is permissible in them, and the same absence of constraint or *prescribed* fiction. Deviation from them is permitted, and the group regards the deviator with a whimsical indulgence. They merge imperceptibly, however, into the usages. For if there is present some arbitrariness of insistence upon the precise "way" of the given culture, we approach usage.

In *usages,* the first fictions appear. One must act "as if." This is often an implicit fiction; and, as the usages level, it is only an arbitrary fiction, not a directly false one. We may take the pattern of eating peas with a fork as our type-example. Now we could really eat peas with a fork, a knife, a spoon, a tablespoon, drink them out of the edge of the dish, or pick them up with our fingers. But we have to act "as if" only the fork were the "correct" or "proper" way. Thus, there is an implicit fiction in the usages. But we do get peas that way. It isn't as if we were required to eat consomme with a sieve. It is thus only an arbitrary fiction, not a directly false one, at the usage level. Except for some training in, or catching on to, tribal ways in childhood, correct usage can become as automatic and unselfconscious as the folkways. They cost the individual little, and are little resisted by him. The worst the group will do to the violator is to laugh at him as a ridiculous person, for no one in the group is appreciably hurt by violations. The insistence-resistance lines cross at a point scarcely above the folkways on our total scale of patterns. But, because of the existence of a fiction (if only a tiny and arbitrary one), the three lines have begun their divergence. Personal caprice might lead me to play at eating my peas, plopping them into the air as children do with jelly beans, and making something of a zestful game of catching them in my mouth. An efficiency expert might well show that the spoon is the most expedite, safest, and most aesthetically acceptable instrument for eating peas and conserving at the same time more of the juices. But the group disregards both of these and prescribes the fork as the only "correct" and "proper" instrument.

It is the tiny, initial divergence of the three lines of appropriateness-reference. Because it is so tiny and meets us so ubiquitously, it is probably vastly important in setting the pattern of unquestioning acquiescence in more vital matters. For the extent to which usages permeate every aspect of our living is constantly marvelled at by those who study them—this fork for the oysters, that spoon for the demitasse, a notch in the lapel, three buttons on the sleeve, this costume for the beach, that tie for formal occasions, white wine with the fish, and so on endlessly. In addition, there is the tremendous utility of the usages of predictability and of communication—to the right in traffic (though on the continent they act "as if" to the left were "correct"), a red light for stop, a hand-shake or rubbed noses to indicate cordiality, doffed headgear to show respect, and the sound sequence, p-e-n-c-i-l, to indicate the object I am holding. All gestures, units of measurements, vocabularies, and all syntax are thus elaborate and sometimes systematized fictional constructs, parts of the group's usages.

There is no *objective* basis for any of these meanings. But by all acting "as if" they were true, *we make them become true* (i.e., functionally true, within our cultural frame of reference). Thereby, we achieve tremendous predictive and communicative gains. We thus get a glimpse of the creative aspect of societal fictions. Also we gain insight into the control culture's intolerance. For unless all act as if the fictions were objectively true, they lose their *functional* truth. Conformity thus has its creative, and individuation its destructive aspects!

This is a point over-emphasized by the conservatives of any given *status quo*. It is apt to be underestimated not only by the rebel, but also by the Freudian, the cultural relativist, and the hard-boiled scientist.

This creative aspect holds true on up our scale of patterns. At the mores and institutions levels, the group creates rich moral and spiritual values of great meaning-resonance to bridge the there widened gap between our diverging lines. That these sometimes completely lack objective validity must be put in the perspective of their functional vitality. In our contemporary Western culture, the old frameworks at these levels are rapidly disintegrating, and new ones have not yet been found. The functional sciences must develop their subtle techniques so that one day we may separate legitimate from spurious fiction and validate appropriate ethical and spiritual values inductively—for all men and all cultures. For though the coalescence of the diverging lines remains an ideal-goal, their divergence is situational, and the resulting strains are natural order phenomena. Balances must be determined. Gaps must be bridged with recompensing psychic meanings. But such meanings must eventually meet the imperatives of all three of our diverging lines. They cannot indefinitely remain pumped-up over-protestations of a single line of validity, coercively enforced. They will always be subtle and complex meanings of great intellectual and emotional resonance. But, as in the individual personality, deviousness (or the lack of it, simplicity) is in itself no measure of adjustment.

Already, at the usages level, we see the inevitability of fictions. If we did not act as if some one way (whether to the right or to the left) were the only "correct" way to turn in traffic, what a mess we would be in. If we limited our vocabularies to words that truly had an objective relationship to their referants, we would never get beyond "bow wow" and "cuckoo." It is when language frankly embraces the fictional principle in the arbitrariness with which referants may be assigned, that it is set free to realize its potentialities as a plastic communicative tool.

In some areas—vocabulary, gesture, etc.—the societal fictions *may* be completely arbitrary. (We show respect by removing the hat, the Hebrew by leaving the hat on, the Mohammedan by removing the shoes. Twiddling the fingers while thumbing the nose could equally well be used, *if* a group agreed to act "as if" that indicated respect.) There is no question but what a certain potential arbitrariness carries on to the patterns higher in our scale, though within sharper limits, as more vital factors are concerned. But quite absurd mores and institutions, once thoroughly established, show a remarkable vitality, and are made part of a going societal integration.

Not that, even at the usages level, more appropriate fictions than those used cannot be discerned. A tribe that tells directions by upstream and down-stream (somewhat as we come to speak in terms of uptown, downtown, and cross-town), or which tells time in terms of the reign of its chiefs, might better (at least after contact with the external world is established) use the less tribal and more nearly universal communicabilities and predictabilities of the points of the compass and the movements of the sun. The establishment of a global culture at the usages level, fictions and all, should not be too difficult. It would be highly desirable in the more important of them, such as a *lingua franca*, common currency denominations, units of measure, calendar, etc. The fictions of a decimal system have an obvious advantage over more cumbersome systems, and it would not be hard to improve on our present calendar. Thus, when we note the functional "truth" of fictions within their cultural frame of reference, we should not confuse this with full functional appropriateness. Many ethnocentric prejudices are functional "truths," in our use of the term, without being fully adjustive in their functional significance. Some are even maladjustive. When refinements can coalesce our diverging lines or establish a better harmony between them, the involved fictions are preferable to the old ones, functionally, even if they are no more "true" than those they replace.

We see from the usages that the group will accept truth or fiction with equal ease, on a pragmatic basis, depending upon which works best, even if the fictions involved are absurd, nonsensical, and quite impossible. Take the usages of time, for example. It is, let us say, either the same time at all points in space at the same moment, or else (taking the sun as a point of orientation) time differs inch by inch as we go from east to west across the map. But neither of these is a wholly *feasible* truth. For the one would give you all degrees of daylight, twilight, and darkness by the same verbal symbols; and the other would require you to carry calculus tables with you and go through endless computations to keep the simplest appointments. So we compromise. We act "as if" time stayed uniform through a given stretch of the map. Then it jumps a whole hour. After another stretch of distance, it jumps another hour. At a given point it jumps a whole day; so that one could stand there and jitter-bug back and forth into yesterday, today, and tomorrow. Absolute nonsense, absurd, and impossible! Yet we are sure that none of you will have such a high regard for objective truth as against functional fiction that, after reading this, you will smash your watches and start carrying calculus tables. Objective truth is greatly over-estimated by scientists and philosophers who reify and absolutize the inductive culture's contained ideal. What good is objective truth to a functioning entity if fiction will work better? It is because fiction will not always work better, and objective truth therefore comes to have a long-run adjustive, workability value, that objective truth was ever given importance by intelligent organisms and societies—and very reluctantly!

The human and social sciences, as concerned with functioning entities and functional structures and processes, will certainly have to elevate the analysis and understanding of fictional "truth" to a par with their quest for objective *truth.* One of the objective truths which they will discover is that functioning entities go along largely on a trial-and-error, pragmatic or workability basis in the process of adjustment-seeking and realizational striving. Lashley and Franz discovered this at the neurological level in the equipotentiality and vicarious functioning of the neural organization. Thorndike discovered it in the "success" factor in learning. The gestaltists have discovered it in the perceptual process. The psychoanalysists have discovered it in the "life lie" and numerous other mechanisms. It is time the social sciences realized the full import of the pragmatic-functional principle in social phenomena.

Thus, from the usages we have learned (1) the inevitability of the use of fictions to gain social predictabilities, economies, communicabilities, and uniformities; (2) their potential sky-blue arbitrariness; but (3) their susceptibility to correction and refinement in terms of smoother functional appropriateness; (4) the preferability of obviating fictions where it does prove possible to "coalesce" our divergent lines; (5) the pragmatic basis on which societal fictions are adopted and hence (6) tend (because of the relation of "workability" to learning and to perception) to be reified as objective truths; and (7) the creative, enriching, and realizational potentialities which fictions carry, along with (8) their stultifying, blocking, and maladjustive potentialities. All of these things are also true of the fictions involved in the patterns higher in our scale—of conventions, mores, and institutions.

Their significance becomes at once graver and richer as those more vital areas are concerned. But it is well to see these things first at the usages level, where we can still be relatively detached and objective in our analysis. For objectivity is no less important in functional analysis than in the analyses of inorganic science.

We shall find that the discoveries made at the usage level hold good on up the scale, with successive accretions at each step which hold good in turn, etc. Thus, there is a usage *aspect* to conventions, mores, and institutions; also conventional aspects to mores and institutions; moral aspects to institutions, etc. For example, take the usage aspect in institutions, namely the involvement of an arbitrary "as if." Just as one could eat his peas with a fork, a knife, a spoon, a tablespoon, etc., but the cultural pattern requires that he act as if only the fork were the proper instrument. We now know, thanks to history and anthropology, that a group may have a perfectly workable order based on monogamy, polygyny, polyandry, concubinage, and so forth; but a given culture requires its members to act as if only monogamy, for example, were proper. Similarly, it might have private property, kinship property, communal property, etc.; or autocracy, democracy, oligarchy, dictatorship, etc. But we are required to act "as if" only private property, or only democracy have validity. And since much more important matters than the eating of peas are involved, the conformity insistence is so much the greater. We do not imply that the formation of institutional patterns is ever as unrestrainedly arbitrary as the usage patterns can sometimes be. But the recognition of their usage aspect is a good way to clear the ground for funtional appraisal of them. It opens the door to the possibility that developed expert appraisal might, some day, indicate an institutional "spoon" as really more appropriate for us than the sacred "fork" we all revere and idealize.

At the *conventions* level, a directly false "as if" is involved. Here, for the first time in our scale, the lines diverge sufficiently that one may refer to a conflict of interests between group members being involved. It is important for anarchists, followers of Rousseau, and those who over-emphasize Fromm's spontaneity principle to see that, even where, as at the usages level, no intra-group conflicts of interest are involved (or even if one assumed an Utopian heart of gold in every group member), the artificialisms, disciplines, and restraints of the usage fictions would still be necessary in terms of the predictabilities and communicabilities necessary for group living. But if there is some strain between spontaneity and total-group needs, there is no involvement of intra-group conflicts between specific persons in the usages. At the conventions level, however, potential personal antipathies and clashes intrude for the first time. The conventions are thrown up as an accommodative device to handle them and keep them within bounds. The conventions thus reflect a higher degree of both insistence and resistance and a wider divergence of our three lines of meaning reference. They thus come a step higher on our scale, and necessarily resort to a greater fictionalization of behavior in consequence. These are interpretive principles to carry with us in our further analysis. For we shall see that mores and institutions are accommodative devices thrown up to handle still more vital conflicts and obstreperous impulses; and, in consequence, they make recourse to still greater degrees of fictionalization.

The directly false "as if" of the conventions may be illustrated by the type-example of a guest leaving a party which he hasn't enjoyed. Let us assume that he didn't want to go in the first place, but it was his boss who was giving the party, and his boss is a great bore socially. They played bridge, which he detests; his partner kept trumping his aces; the wine was flat, the jokes corney, and the sandwiches mouldy; and the hostess had halitosis. Now, he is at the door, saying goodnight. What *must* he say? Explicitly or implicitly, he must carry out the directly false fiction that he had a lovely time, is glad he came, and hopes to come again soon. And, though the host only invited him for conventional rather than spontaneous reasons and was himself bored stiff by the whole affair, he must respond in kind, "I'm so glad you could come. It was a pleasure to have you. Do come again soon." And if he hopes never to see the guest again, he just might add, "Don't wait for an invitation. Come any time." Thereby he warns the guest that he will not receive further invitation, but permits the face-saving interpretation that the lack of them derives from social closeness rather than social distance.

For the language of the conventions is devious. But, as we have seen, deviousness is not in itself an index of adjustive inappropriateness. The tribal member must learn to thread an intricate way among the deviousnesses of the tribal conventions. It is a blunder, ranging from comic boorishness to pathetic or really tragic consequences, to take the conventions at their face value. Functional truth and objective truth, from here on up our scale, are often exact opposites. Straining at exact-science objectivity in these areas thus become stultifying. Recourse must be had to the methods of insight, convergence of inference, and functional analysis.[6] The social psychologist, sociologist, and anthropologist have to distinguish between (a) the objective conventional response and what is actually communicated, at the conventions level; (b) the ideals or professions and the actual practices and counter-mores, at the mores and institutions levels; and (c) between even the achieved practices in turn and the frustrated and repressed tendencies still active below the surface. These problems are especially difficult for the anthropologist, who must know the language, live in the group he is studying for a considerable period, and bring a sufficiently sophisticated sociological, psychological, and psychiatric background with him to be aware of all the nuances of what really goes on. In these terms, considerable tenuousness must be attached to anthropologists' social psychological and social psychiatric findings, even quite recent and sophisticated ones, and even those worked out in collaboration with the psychiatrists. Anthropologists are becoming more aware of these hazards. Sociology is a little slow in catching up its methodological lag.

Volumes could be written on the ubiquity of the conventions and the extent to which they infiltrate into every aspect of social living, from the closest intimacies of friends to the widest ranges of society; from the triviality of a polite response to the gravity of a diplomatic announcement upon which world peace itself might hinge. Someone steps on your toe, says they are sorry (though they are not), and you respond that it didn't hurt at all (though it did). A cabinet minister is really fired, but receives an elaborate congratulatory letter on his splendid services and expressing hope his ill health will not be of long duration. If you had decided life was meaningless and were on the way to the river to end it all, and a friend greeted you with "Hello, Jim, how are you?" you would probably respond, "Fine, how are you, Joe?" and go on and jump in. A whole civilization acts for centuries as if women didn't have legs or as if there were no such thing as sex, leastwise not in the family situation. And so on, endlessly.

The functional character of conventions may be seen by taking our type-example and assuming spontaneous responses instead of conventional ones. Our guest informs his host that he has had a lousy time of it and hopes he will never have to come again. Whereupon the host retorts that he only invited him out of courtesy, because he was a relative of his wife's, and that whole branch of the family are nothing but a bunch of jailbirds anyway. From that point, things begin to happen —disruptive quarreling and feuding, or the breaking off of further social interaction between the two. Thus, the conventions are accommodative devices (a) to enable the continuance of social interaction and (b) to keep the surface smooth in spite of underlying tensions. Their function is the *raison d'etre* of their existence and their characteristics. Mores and institutions are similar accommodative devices to hold even more divisive and intransigent strains in bounds.

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[6] See James W. Woodard, "Five Levels of Description of Social Psychological Phenomena," *Sociologus*, 9, 1, 25-31.

The occurrence and types of conventionalizations can be predicted in terms of their functions, as thus described. Empirical observation will bear out such *a priori* analyses. Thus, the degree of formal conventionalization of social interaction will depend upon three sets of factors: (a) the importance (+) or unimportance (—) of continuing interaction and of maintaining smoothness of interaction; (b) the number and intensity of strains and conflicting interests which threaten to disrupt interaction; and (c) the number and intensity of spontaneous bonds, whether in interest or affection, whose existence obviates the necessity of recourse to formal conventions for cohesive smoothness. In terms of (c + to c —) for example, conventionalizations slough off as two people get better acquainted and become closer friends. Their relation changes from a conventional to a personal relationship. If one wishes to avoid having a relationship become personal, he retains the conventional forms of interaction. Or if a personal relationship becomes strained, a retreat into conventionalized interaction often occurs; and sometimes this bridges the period of strain smoothly enough to save the relationship. For conventions are depersonalizing devices, as any device must be which is to accommodate diverging centers of meaning-reference. It is to this depersonalization that they owe their efficacy. One acts "as if" one's personal reaction did not exist. Thus, the extent of conventional formality is a measure of social distance. Razzing, e.g., is usually a sign of social inclusion rather than exclusion, its functional character being the reverse of its superficial or "objective" appearance. There are far fewer formal conventions in primary groups and rustic neighborhoods than in secondary groups, at the king's court, or in cosmopolitan circles. Overlooking the relativities of appropriateness, poets and moralizers have often idealized, and clergymen are constantly nostalgic for the simple honesty of rustic ways by comparison with the more devious urbanities of the latter groups.

We have accounted for the three-fold structuration of the culture in terms of the combinations and permutations of three sets of factors: importance-unimportance, acquiescence-resistance, and provability-unprovability. And we have accounted for the ascending scale of patterning within the control culture in terms of the combinations of group insistence and individual resistance, the increasing divergence of two of the lines of meaning-reference. In the same way, characteristic changes occur in the patterning of the conventional fictions with every combination and permutation of factors (a), (b), and (c) in the preceding paragraph. Space does not permit full elaboration, but we may take one or two examples. There are (b +) many rivalries, personal animosities, and clashes of interest in a university community. At the same time (a +), it is important that relations between the president, the deans, the department chairmen, the teaching staff, and the student body never be broken off, but be kept as smooth as possible. Personal reactions must be subordinated to the imperatives of the institution and its functions, resulting in a hierarchical order of conventionalizations and social distances within the university community. A teacher should show a personal interest in his students, but it must never become *really* personal—to the extent, say, of quarreling oı discriminatory flunking on the one hand or overt expressions of affection on the other. One does not say, in faculty meeting, "Professor B is a stuffed dodo and ought never be entrusted with the chairmanship of this committee." He says, "We have the highest regard for Professor B, and think he would be an ideal chairman; but in terms of precedent, etc., etc., we think the chairmanship should be vested this year in Professor A of the X department." Students may achieve a relatively spontaneous and frank interaction among themselves and academic colleagues may approximate that. But each such stratum in the university community is insulated in some measure from every other one by a host of conventional fictions and depersonalizing and social-distance-maintaining devices. This is extremely stultifying, but equally necessary, at least in some aspects. God help the university teacher who ever tells either his students or his superiors what he really thinks! One might think that a faculty reception, the assemblage of so many brilliant minds and creative thinkers, would be a most stimulating occasion. It is perhaps the stuffiest occasion known to man.

The conventions do not entirely inhibit aggressive reactions. Sometimes they instrument them. A lifted eyebrow or a modulation of the voice can inflict a more exquisite wound within the conventionalized framework than the frank smash on the nose or fishwives' hair-pulling that occur at less refined cultural strata. The relativity of personality in the same person can be seen, too, in his contrasting behavior at a formal occasion, at church, on a pleasure cruise, at business, or in a crowded subway. For different fictions are imposed, and followed out, in these different groups, in accordance with their different conditions, societal functions, and contained imperatives. In the subway, for example, (a —) there is no particular importance in continuing interaction or in keeping it smooth. That particular combination of persons will probably never be together again anyway; and (c —) there are very few spontaneous bonds. Therefore, subways do not throw up many conventions of their own. They get along on the good breeding which seeps into them from the general society. This tends to degenerate into bad breeding in the subway climate, since the organic supports for good breeding are there lacking. Accordingly, (b) the only item of strain, the desire for seats and standing room, comes quite frankly into the open. We have seen the very same person who was so much the soul of courtesy at the faculty reception that you would think it was a pleasure and an honor to have his toes stepped on, become the soul of boorishness only a few moments later in the subway, "Whadda ya want, the whole car?" On the pleasure-cruise, (a —) there is not much importance in continued interaction and its smoothness, for few will meet again after the cruise is over. (b —) There are also few conflicting interests, and these are of low intensity. The pleasure-cruise, too, gets along on the good breeding that hangs over from the general society. But this alters in the new climate in the direction of the permissible release of (c) the major common interest, enjoyment. Hence, the pleasure-cruise, like the night-club, the annual convention, and the New Year's Eve celebration, tends to set up its own fictions and conventions—amoral or immoral from the standpoint of the general society—the mores of dalliance (the summer flirtation) and the conventions of irresponsible enjoyment. Anything goes.

The extreme of formal conventionalization is of course found in the situation where (a ++) it is of life-and-death importance that interaction continue and smoothly; (b +) there are many conflicting interests and obstreperous urges threatening disruption; and (c —) there are few or no spontaneous bonds. Of course such importance, conflicts, and resort to formalism carry us beyond simple conventions to the conventional *aspects* of institutions. The military fighting unit answers this description, and military courtesy is possibly the extreme of conventional formalism and fictional depersonalization. Neither is it without significance that, as common experience in the face of battle-fire welds spontaneous bonds, the peccadillos of courtesy tend to slough off. A stern requirement for green troops (the Russians tried to do away with military formalism after the revolution, but found they had to reintroduce it), precise military courtesy, is a "pain in the neck" to the veteran on leave from the front and thrown into contact with those for whom it meets different functional imperatives. The elaborate fictions and depersonalizing conventions of legal and juridical procedure and those of parliamentary procedure are also examples of this type. But perhaps the extreme of deviousness and finesse in conventionalized fictions is reached at the diplomatic table. Here (b +) sharply clashing political, economic, and military interests have occasioned the conference; (c —) the ethnocentric patriotic and nationalistic fictions have precluded all but a slender thread of spontaneous bonds, and (a +) war or peace, victory or defeat, perhaps national survival itself depends upon the continuance, and the smooth continuance, of negotiations. Accordingly, diplomacy erects the most intricately conventionalized and fictionalized procedures. Diplomacy consists in part in saying what you don't mean; in managing to convey what you do mean; in acting as if the manifest content of what you hear were all that had been said; in detecting correctly the latent and real content between the lines; in letting the other know that you get what he has really meant; and, all the while, in neither side breaking face. Polite society is sometimes like that, too.

This is not hypocrisy, as the politically or socially untutored think. It is functional deviousness, hit upon in response to very complex situational imperatives. Any woman will tell you that, in our culture, the woman's conventional behavior in courtship must be of this devious sort—pretending innocent surprise, stimulating by resistance, and resorting to inviting retreat. All the charm of coquetry and much of the freshness and exquisite travail of young love flow from the deviousness which our romantic and moral fiction put upon the love impulses. Woe to the swain who cannot thread his way correctly between the "no" on her lips and the "yes" in her eyes. And woe to the over-moral lover who mistakes preliminary nuances for cruel betrayals or deathless pledges. For these are not hypocrisies either. They are functional deviousnesses threading among very complex imperatives indeed.

Cultures differ in the extent and character of their conventionalizations. The literal honesty of the Quakers, for example, or the blunt but not ill-disposed frankness of the Pennsylvania Dutch might be contrasted with the elaborate conservation of personal dignity among the Southern aristocracy to illustrate this. Perhaps the peak of conventionalization is reached in Oriental face-saving, which is almost a basic principle of the societal organization itself. In our culture, few formal conventions characterize our primary groups. But among primitives, because of the venomous intensity which personal relations can reach in an *exclusively* primary-group universe, because of the importance of personal status in tribal relations, and because of various elements in their belief systems, personal interaction is sometimes extremely conventionalized and ritualized. In our culture, formal conventionalizations slough off in the family, but in the older European family, even intra-familial interaction was highly conventionalized. In the old Chinese family, it was extremely conventionalized. This brings us to our final example of the permutations of factors influencing conventionalization, i.e., when all three factors are high.

We are considering the case where (a +) it is extremely important that the functioning unit be not disrupted but must continue to function smoothly; (b +) there are grave strains and conflicts of interests; and (c +) there are *also* strong common interests and strong spontaneous bonds of affection. Such importance in both common and conflicting interests implies a high insistence-resistance level and is the stuff of which mores and institutions are accommodatively fashioned. The presence of strong and spontaneous affectional bonds in addition, implies one of those organically rooted, primary-folk institutions which perform a permeative, pervasive, and vital sustenance function, materially and psychically. This it does for its constituent members at its own level of integration, and as an entering unit into the larger societal integration. The family is preeminently of this type. So, in less degree, are religion and the primitive tribe-as-a-whole (scared society).

The other institutions and secular societies are somewhat less so, since the (c) factor, affectional bonds, is somewhat altered in them. Taking the family as an example, we have seen that because of its personal and whole-personed character, formal conventions tend to slough off. But informal, implicit, and "latent" fictions of the conventional type increase in number and in the gravity of their import. This is probably true even where recourse is had to considerable formal conventionalization, as in the examples cited. Because of its own contained imperatives—in order that it may continue as an interacting unit, function smoothly and without disruption, and perform the functions delegated to it by the larger society—the family must act (1) as if the fictions involved in its more vital socially delegated educative functions were actually real rather than only functionally accommodative; (2) as if sexually-toned love, with its disruptive jealousies, discriminations, etc., did not exist in intra-familial relations; but (3) as if filial, parental, and brotherly love (de-sexed or sublimated "Christian love") existed, and was the only strong emotion which did exist and have *essential* validity; (4) as if serious intra-familial aggression did not exist or have natural-order validity; but (5) as if common loyalties and common identifications did. The family must so thoroughly act as if these things were true, as a developmental milieu for the growing generation, that it makes them *become* true (functionally, in the clinched attitudes, behaviors, personalities, and characters of the interacting members).

In order to do so thorough a job at such an obstreperous level, it cannot afford to admit that the involved fictions are fictions—they must be taken as actually real! Deviate impulses must be denied their natural order legitimacy. Insight into the involved functional deviousnesses must be prevented. Discrepancies with objective reality must be repressed from conscious awareness.

The fictions of the *mores and institutions* level thus reflect the greater divergence of our lines of reference, and carry on accordingly. There is (1) a greater elaboration of formal conventions, the conventional "aspect" of mores and institutions already noted (e.g., military, juridical, parliamentary, diplomatic, and sometimes even familial courtesy). There are (2) the fictions and conventions which surround the institutionalized role—the priest, teacher, judge, prince, military officer, parent, etc., from the lowliest functionary to the Grand Lama of Tibet. Here are the endless things which, at certain ages, children do, but keep from their parents; that parents do, but not before the children; that parents both do and require their children to do because the community, rather than their own spontaneous responses, require them, etc. Here is a greater profession of interest in things moral and spiritual than they really feel, with which the flock of the minister constantly holds up the mirror of their expectation of *him*. They act, when in his presence, as if only one of the many segments of themselves and of life had validity; and they require the person in the institutionalized role to act as though he were the personification of the control culture's values. By the time the group's pressures, the role's contained pressures, and the reificatory tendencies which flow from such consistencies in conditioning, expression, and workability are through, the highly institutionalized personage is a hand-carved instrument, a veritable "organ" of the group, for carrying out his delegated functions. He becomes quite capable of flunking his students out of school and ruining their career chances if they persist in splitting their infinitives; of bidding his daughter never to darken his door again if she will not show obedient respect to her elders and quit loving that man; of consigning his parishioners to eternal fire unless they are baptized by immersion; of consigning the rebellious to political prison or the firing squad; or of directing the torturing on the rack and the boiling in oil of those who refuse to believe. There are (3) the fictions and conventions which surround delegated roles and functions other than the strictly institutionalized roles—e.g., the culturally defined nature, role, and status of women—or of the Negro! There are (4) the fictions involved in the rationalizations and ideologies of the group, wherever these make use of fallacious material and modes of argument. This they must often do, from one culture to another, in order to induce wives to obey their husbands; slaves to obey their masters; widows to throw themselves on the funeral pyre of their husbands; maidens to go from thirteen (pubescence) to thirty-three (when they may have finally married) without sexual functioning; youths to give an arm or a leg, or life itself, in war, etc. There are (5) the implicit fictions in the ritual, ceremonial, pageantry, music, poetry, song, and story whereby emotional persuasion to the group's values (rather than the intellectual persuasion of the interbuttressing rationalization systems) is achieved.

Of these fictions at the mores and institutions level, many are true conventions, or conventional "aspects," as we have noted. But (6) the most important fictions at these levels are informal and implicit.

They are reified by the moral self; the conflicting aspects of reality are denied natural order validity; and contrary aspects and disruptive urges are repressed from conscious awareness. This is not merely because of habit, workability in the functioning framework, and the like. The group requires it. At the usages and conventions level proper, the group only requires conformity in behavior. It is quite willing for the individual to see through the involved fictions, as is also true in the appreciative-expressive and inductive cultures. Guests are no sooner out of ear-shot of the host than they are telling each other, within prudentially conventional limits only, what kind of an evening they really spent. But at the mores level, the moral "ought" is attached for the first time on our scale. Adherence is thought to be so important, and the disruptive individual impulses are so strong and so eager for rationalizations to warrant deviant behavior, that insight into the fictions and awareness of the discrepancies with what really goes on cannot be risked. Or so it is thought. The group now requires *belief* as well as conformity.

In fact, in some aspects, the group requires belief more strongly than it does conformity itself! One may violate, but if he conceals his violation (what a train of fictional disparity between ideals and practices flows from that fact!), if he is duly punished, if he confesses his shame and goes through prescribed restitutions and purifications, or if he but still feels guilt, the *framework* continues to stand. Not so, if one deny the framework itself. Professor X, and Dean Y, and even President Z (all actual cases) have had mistresses for years—but discreetly; and they always protest, indeed compensatorily over-protest, the ideals of our monogamous family. But if Professor R has an ideal home life, but calls the *framework* into question, he may not teach our American youth—not even mathematics! A Sam Insull or a Richard Whitney can do their penance and return to society, even secretly envied by many. But let the communists and atheists watch out! Every society has its modes of forgiveness and reinstatement for violations (its fines, penances, restitutions, prayers, ablutions, purifications, and even its accommodative counter-mores), so long as belief in the framework is professed. But rejection of the fictional framework is the unpardonable sin in all societies. It was not for stealing or unchastity that the Inquisition tortured and killed its victims. It was for disbelief. It is not thieves, grafters, profiteers, or prostitutes that are shot or put in concentration camps in contemporary Europe. It is those who challenge the framework itself. "He who doth not believe, let him be anathema." The situational imperatives to this, and the creative and even spiritual potentialities of the group fictional consensus, of which we have earlier spoken, are hard to keep in mind when ranged against the stultifying, adaptation-blocking, and tyrannous aspects of such examples.

One way in which the group achieves this reification[7] of its fictions (and repression of the contravalent awareness and urges) is through a so-called "conspiracy of silence" (not that it is a self-conscious conspiracy necessarily). It is interesting that complete silence, acting "as if" the item in question had no existence at all, indicates greater disapproval and stronger taboo than when its existence is admitted, apologized for, rationalized away, attacked, and so forth. This can be seen even at the conventions level. It is in these terms that the society's and the family's silence on sex, especially in its incestuous and perversion aspects, results in such a horrendous anxiety and deep personality conflict. Children, at certain ages, refuse to believe that their parents have intercourse. Other parents might, but not theirs; or, grudgingly, perhaps the once that was necessary for the child's birth. Even late adolescents or mature people are unable to visualize intercourse between their parents. Confronted with the extreme taboo and horrendous mystery which the "as if of silence" imposes, the individual is stunned by his first awareness of sexual, incestuous, or perverse impulses. He thinks he must be some exceptional sort of monster, and he sharply represses the impulses and awarenesses from consciousness. Due to the extent to which deviations of all kinds are always concealed, and ideals professed and even compensatorily over-protested, this is implicitly true of all mores and institutions, and especially so in the religious and familial areas. Mothers, since time began, have had the evidences of infantile sexuality before their very eyes. It is easier, however, to recognize fiction (as "primitive mentality," say) when certain primitive tribes persist in believing that dead bodies do not cast shadows, in the face of the factual evidence. But even sophisticated secular societies can carry such "blind spots"—areas of extremely effective fictional silence and concealment. It was a great shock to the Western world when people like Machiavelli, Darwin, Marx, Freud, or the "muckrakers" at the turn of the century disturbed some of the vital fictions we were living by, to show us some also present realities.

There is considerable inconsistency and strain between the fictions in the various parts of the same culture, especially in complex, secular societies. The three parts of the culture have their differing fictions and "contained" ideals. Educational, religious, military, juridical, governmental, and welfare institutions are under considerable strain with each other in Western society at present. Each delegated role, profession, occupation, interest group, or other social fracturation due to the division of labor, *tends* to do likewise. This is inevitable in view of the "center of reference for meaning and value" which is implicit in all functioning and group formation. Each cultural stratum, class, socio-economic level, and ecological region tends to diverge similarly in its "way of life" and the involved supporting fictions. So does each culture, national grouping, formal political state, and the like, taken as a whole. Within each culture (even simple and sacred societies), there is always a strain between (1) the organizational mores (and their supporting conventions and usages), (2) the humanitarian mores, and (3) the "counter-mores" which support the actual practices in their disparity with the ideal professions of the group.

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[7] On reification, see James W. Woodard, *Intellectual Realism and Culture Change*, Sociological Press, Hanover, N. H., 1935.

Thus, the pattern of strain between conflicting ways of life and their supporting fictions, often idealized, reified, and sanctified, becomes extremely complex, now that transportation and communication are welding the entire globe into the interdependence of the Great Society. By the same tokens, the strain toward an integral framework of meaning, giving sense to the various parts, and setting divergences into understandable perspectives, is the great intellectual strain of our time. The old tribal frameworks are rapidly disintegrating, and a new, self-validating, and universal framework has not yet appeared. Neo-Thomism, the educational proposals of Mortimer Adler, recourse to force and propaganda, and the philosophies of dictatorial totalitarianism are attempts to achieve that framework by force, or by regression to frameworks which once sufficed. But the historical imperatives of the past and those of the future seem apt to be in so many ways almost diametrically opposite,[8] that such attempts can have at best a short-lived success. They must fail as the Inquisition failed, only much more quickly, since science, transportation, communication, and the other individuating forces were only in embryo at the earlier period. The sole long-run hope is in a genuine merging of the global collective interests and the erection of an integral frame of cultural meaning-reference. Both of these depend in turn, in no inconsiderable measure, on the extent to which the human and social sciences embrace their functional character and begin to give us adjustive *wisdom* as well as sheerly factual description. That is our strategic reliance for the future.

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[8] See James W. Woodard, "Cultural Evolution and the Social Order," presidential address before the Eastern Sociological Society, in *Jour Soc. Phil* , **6** (7) 313–326

The economic interdependences of modern times are a transitionally disruptive, but long-run integrative factor. The inductive culture is in its very nature potentially universal in character. The utility usages of the control culture have a similar universal potentiality. The appreciative-expressive culture and the folkways proper may, in terms of their low insistence-resistance level, permissibly vary with enrichment and enjoyable variety accruing. The humanitarian mores, in the control culture, occur in all cultures and have a contained strain toward intercultural or universal validity. But the rest of the control culture, from conventions on up, varies viciously, becoming reified, sanctified, and defensively or evangelistically bellicose. This follows from taking the involved fictions as actually real. Societies will have to continue doing this until the functional sciences can yield some sure-footedness to adjustive relativities. The human-social sciences cannot do this, in turn, if they keep straining for inorganic-science objectivity; or if they are merely pietistic, moral, nationalistic, or adopt some single center of functional reference, as, for example, does German geopolitics.

There are certain key fictions which more or less characterize all control cultures in *past* history. Their creative and integrative utility must be fully realized; and at the same time their tribal character, their blocking of inter-cultural adjustment, and their stultifying influence and sometimes inhuman consequences must be frankly faced. These key fictions are: (1) That there are gods, although the convergence of inference from biology, psychology, psychiatry, sociology, anthropology, and philosophy is that there really are no gods. The gods are tremendously vital societal projections; important functional fictions, but still fictions. Several corollaries follow: There are gods, and (a) our tribe's god is the true god; (b) we know the god's will; and (c) he sanctions our tribe's ways and strengthens our cause when we fight other tribes with other ways and other gods (or even with the same god!—for the gods are always peculiarly *ours*).

(2) That right and wrong are absolutes and eternal verities, although the convergence of inference (again from biology, philosophy, but especially from psychology, psychiatry, sociology, and anthropol-

ogy) is that morals and the concepts of right and wrong are group-situational emergents. Morals reflect, on the one hand, the universal values situationally imperative from the nature of group living as such (such as the humanitarian mores and the mores of reliable predictability); on the other hand, they always reflect the endless tribal relativities of morality which flow from the differing environmental and historical imperatives and the differing centers of reference for meaning and value of the groups which throw them up. Similar corollaries follow this fiction: Right and wrong are eternal verities, and (a) our tribe's ways are right; (b) where the ways of other tribes clash with ours, they are wrong, and (c) eternal righteousness is on our side.

(3) That the individual is a free-will agent and could follow this right if he would, although the convergence of inference from the same cluster of sciences is that man is a part of the cause-and-effect universe. This is true however much his determination is an "organismic" one (permitting the use of reason as, in turn, affected by hereditary needs, past experience, emotional fixations, repressed conflicts and distorted symbolisms, cultural frameworks, social imperatives, etc.) rather than a detailed specific inorganic-mechanical determinism. Corollaries flow from this fiction in turn: People are free-will agents and could do right if they only would, and therefore, (a) punishment (*as* punishment, not as the involved disciplines of training, rehabilitation, or therapy) is deserved by the individual; and all the sadism that is poured into punishment from petty stigma to the mass tortures of wars and inquisitions are therefore justified; and (b) the feelings of shame and guilt are also justified, with all their travail and lonely ache and sometimes suicidal mosochism.

(4) Most cultures also hold, somewhat as corollaries of (1) and (2) above, that the tribal morals and institutions are pure values, ends in themselves, rather than seeing them as accommodative devices to be altered, scrapped, or replaced when they no longer function properly or when changing imperatives call for different accommodative devices.

(5) The fictions involved in the concept of loyalty, especially loyalty to the in-group, are also universal among control cultures.

The pattern of stigmatization and praise varies up our scale and reflects the greater and greater insistence-resistance tension of the more and more diverging lines of meaning-reference. The deviator from the folkways ("violator" is here too strong a term) is regarded with whimsical indulgence. The violation of the usages is regarded as ridiculous; that of the conventions as uncouth and in bad taste; that of the mores as wicked, bad, and sinful; and that of the institutions as criminal, unnatural, abnormal, and monstrous. These stigmatizations are reified by the tribal member with corresponding feelings ranging from chagrin to suicidal guilt. These stigmatizations are cumulative, the violation of the mores being in bad taste as well as wicked, that of the institutions wicked as well as criminal and unnatural. Objective punishments parallel this series, ranging from a tolerant smile or hand-slap to banishment or electrocution. Correspondingly, rewards to the conforming member's self-feelings are bestowed, ranging from the certification of his propriety to deeply resonant moral and spiritual values. These are psychic rewards and punishments which, together with the objective rewards and punishments, help to bridge the gap between the more and more diverging lines and to make the group way of life the actually real world of meaning for the individual (as it thereby *becomes* even in, say, cannibalistic societies as a framework of limitations, workabilities, and practical realizational possibilities).

The increasing stigmatization and laudation, and the increasing punishment and reward reflect, of course, the increasing pristine resistance as well as the group insistence. So does the fact that the usage stigma—ridiculous—fails to carry up. The violator of the mores or institutions may be wicked, etc., but there is too much vicarious identification with him and secret admiration of his courage, if not of his morals, for him to be ridiculous to any but the most naive of the tribal members. The mounting resistance is also reflected in the epithets which the self-conscious rebel, in any given culture, hurls at the patterns themselves. He stigmatizes them for what they are if we overlook the functional character of their fictions, and for what they become even functionally, when they cease to have historical appropriateness. Thus, the self-conscious rebel stigmatizes the usages as meaningless; the conventions as hypocritical; the mores as inhuman (e.g., the Hester Prynne situation in the *Scarlet Letter*); and the institutions as unjust. These epithets are also cumulative, again with the exception of that of the usages—meaningless.[9]

It is apparent that, while the rebel can stigmatize fork-preference for pea-eating at the usages level as meaningless, he will have much less success in stigmatizing as meaningless the fictions of standard time, weights and measures, vocabulary, and the like. Here, their functional utility precludes any meaninglessness. Several considerations follow. What the person is really doing in using the fork for his peas is indicating his social status—that he "belongs" to that tribe or class level which carries the given definition of propriety. His chagrin in lapsing into the use of the knife or his fingers is not in the functional inappropriateness of those instruments but in the betrayal that he really doesn't belong. Thus, examples of the various patterns up our scale will fall into two groups: those that have been thrown up predominantly by the forces at the given level and which are susceptible of functional justification at the given level itself, and those which predominantly derive from, or help buttress, patterns higher in the scale and whose functional justification will to that degree be contingent upon justification of the pattern from which they derive. Thus the usages of utility (communicability, predictability, etc., as in vocabulary, gesture, units of measure and the like); the conventions of human consideration and of a finer art of social interaction; the humanitarian mores and the mores of reliability (honesty); and the welfare institutions (whether protective, disciplinary, ameliorative, or creative-realizational in their welfare emphasis); these have been thrown up by the contained strains of their own insistence-resistance levels, and can be justified or subjected to refinement and correction at their own levels in turn. But the usages of status or of feminine propriety; the conventions of modesty or courtship, or those surrounding the specific institutionalized roles; the mores of chastity or those involved in the particular property forms; and exploitative institutions such as slavery, caste, and some religious, governmental, and property forms; these cannot be justified at their own level alone. They are derivative patterns, and their functional justification depends also upon the functional validity of the patterns they derive from and which they help buttress.

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[9] In terms of precise definition, the *folkways*, are culturally carried patterns of behavior that involve so little tensions and disparities that they are all but unselfconsciously followed, true group habits. The *usages* are cultural patterns which involve an arbitrary "as if." The *conventions* involve, in addition, directly false fictions. The *mores* (still cumulatively) further invoke the moral "ought," rationalizing ideologies, emotionalizing rituals and ceremonies, and super-rational sanctions. The *institutions* add to all these the additional factors of an organization and personnel to enforce and propagate the behaviors and involved values and beliefs.

With reference to the exploitative institutions just mentioned, it is most important to point out that this whole series of derivative and cumulatively buttressing patterns and fictions is subservient to the power, prestige, and wealth factors in the class structure and the *social* organization in turn. This deserves volumes of comment and intricate analysis. But we are here confining ourselves to the cultural factors *as* cultural.

The group defines "truth" and "morality" pragmatically, in terms of workability from its own (or, historically, often from the ruling class's center of reference. But it will not stand for it if the individual uses truth or falsity with equal readiness, depending upon which works better from *his* center of reference. That is lying, and is wrong. When the inspirational leaders of the tribe speak of devotion to Truth, with a capital T, they do not mean the truth of science. Nor do they mean any disturbing analysis of the other facets and centers of reference for meaning and value. They mean not truth but the tribal fictions. But the group itself is quite opportunistically pragmatic in this regard. The same group that forbids you to lie about important matters which seriously disturb predictabilities at the mores level, commands you to lie (white lies that keep the surface smooth) at the conventions level, and requires you both to lie and to kill, when an enemy-group is concerned. Morals are a group-situational emergent. As such they invariably carry the implicit in-group center of reference of the group which throws them up, or of the prestige group which can most influence them, and in such pragmatically functional terms. For while the discrepancies just noted are objective inconsistencies, they are functional consistencies. Moral pronouncements tend, however, to be reified and absolutized and to be passed on in such terms. Thereby, in the naivete of the over-conscientious and the conscientious objector, morals set up a strain toward greater internal consistency. In connection with the humanitarian mores, morals thus presently tend to undermine their own ethnocentric and sometimes exploitation-facilitating character, and to undergo change from repressive tribal morals to a positive and universal ethic.

The deep repressions, thwarts, and reified fictions of the mores and institutions level enter most significantly into personality conflict. But the conventions are not wholly without significance in that regard. If they do not involve actual repression, they do sometimes involve inhibition and considerable thwart and frustration, especially frustration of sexual and aggressive impulses. The thoroughly conventional person, like the thoroughly moral one, is apt to lead a narrow and barren existence within a frame of reference that doesn't exactly accord with reality. Yet in a sense it is reality; and if everybody lived out its fictions consistently, the creative aspect of the conventional fictions is capable of achieving a more civilized level of existence in which conflicts are processed smoothly, feelings spared, and new values created—a finer art of life. It must be seen that, up to a point, conventions, and mores and institutions with them, can be thus creative of new values. Beyond a point, they become hypocritical, tyrannous, exploitative and inhuman. If the underlying tensions are grave enough, it becomes ostrichism to keep surfaces smooth by acting as if the tensions were non-existent. To do so is futile, as both psychoanalysis and the history of revolutions show. The societal fictions are potentially both creative instruments and inhuman tyrannies. During the developmental period of a given cultural order, most is probably gained by living out its fictions, living up to its ideals. But in the long course of history, it proves to be as important that men lose their specific ideals as that they live up to them. A vigilante member and a revolutionary party member represent the two extremes. The first has reified the fictions of the status quo, *as is*, making no distinctions among them. All that is needed is to make people, by force, if necessary, live up to them. The second is apt to reject the fictions and ideals of the given status quo *in toto* as "bourgeois sentimentality," or the like, only to find, after he has made his revolution, that he has to bring some of them back, or that he is confronted by a new set, derivative from the new social order. For better institutions can diminish the strain between our three lines, but can never completely coalesce the lines. Therefore, any set of institutions entails its derivative mores, conventions, and usages. If, under monogamy, I must act as if I did not love another's spouse (although I do), under free love, I must act as if I were not jealous or wounded, when someone else makes love to my love object (although I am). And so on.

Traditional morals and religion on the one hand, and science and psychoanalysis on the other tend to take opposite points of view on these matters. Morals and religion tend to reify the absolute character of the ideals and fictions, let the guilt feelings and other maladjustments fall where they may. Functional science and psychoanalysis tend to recognize the natural-order legitimacy of both sides of the ambivalence, to recognize the instrumental and accommodative character of ideals and fictions, to analyze their functional appropriateness and modify them a bit in the direction of reality-contactedness. They thus hope to free man of his age-old burden of guilt, and to engineer the creative and realizational potentialities that lie in our diverging lines, their integral coalescence, or their smooth accommodation, involved fictions and all.

We cannot here go into all the relativities, subjectivities, hazards, disciplining safeguards, and complexities of interfunctional frameworks that are involved in attempting inductive appraisals of functional appropriateness. We realize the difficulties as well as the vital importance of such attempts. Relative to cultural fictions, the generalized criteria are three-fold; (1) provable utility; (2) aesthetic value; and (3) humane consideration. These criteria reflect the contained ideals of the three parts of the culture and the contained strain to assimilate or transform the arbitrary and extrinsic aspects of the control culture so as to yield a more integral framework of meaning-reference. Utility (1) enters at the usages level in the communicabilities and predictabilities established by many of the usage fictions. These utilities are researchable and subject to inductive cultural absorption, involved fictions and all. The same is true of the utility, predictability, and security aspects of conventions, mores, and institutions; but this is more difficultly true, so to speak, and is contingent upon the achievement and successful development of the functional human and social sciences within the inductive culture. The aesthetic criterion (2) enters also at the usages level in the aesthetic usages, and continues on through conventions and the other patterns in those aspects concerned with a finer texture of social interaction and a more highly developed art of living, involved fictions and all. It contains some unifying elements and some elements of enriching diversity. The humanitarian criteria (3) enter first at the conventions level in the conventions of face-saving and human consideration, and continue on in the humanitarian mores and the welfare institutions. The graver predictabilities involved in the mores of reliability and the laws; the security of status (government), security of sustenance and possessions (property), security of love (family), etc.; and the safeguarding of the psychic incomes and material benefits inherent in the group's "way of life"; all these involve our same three criteria at their graver and more complex levels. Social status and social organization are thus seen as legitimately deriving from social function and from the cultural organization, rather than, as in the historic past, the other way round. For there are no other "contained" functional criteria in social-cultural structuration and processing *per se*.

This is important to see, because, in actual historical settings, there have been other criteria of great forcefulness and of a natural-order inevitability relative to the historic situation. We refer to such things as the existence of the gods, the divine right of kings, white supremacy, the sanctity of private property, the natural right of individual enterprise, etc., whereby the control culture has been made to buttress the social organization, the class structure, or exploitative institutional forms. It is not without significance that, when insight into the fictions of standard time is gained, one has no tendency to destroy his watch or to cease acting out the fictions anyway. But when one gains insight into the fictions of monarchy or of white supremacy, he ceases to obey, and begins to plan how to change the social order from which the fictions derive. It is in these terms that one might expect, under the impact of individuation, mobility, social fracturation, the development of scientific scepticism, increasing material interdependence, etc., a long-run trend undermining those merely tribal or arbitrarily historical criteria which run out to the class, structure or to the supposed gods for the sanctioning of inappropriate and exploitative mores and institutions. The *long* future probably thus belongs to the positively *creative* state, rather than to the exploitative, repressive, or merely refereeing *laissez-faire* state—to the society with welfare-based social institutions whose techniques and supporting cultural patterns and belief systems, involved fictions and all, have a functionally oriented inductive culture as their sure-footed foundation.

That such a society would have ethical and spiritual values of compelling meaning-resonance must be quite apparent. Beginning at the usages, and diverging more and more up our scale is a discrepancy between objective or material reality and another "order of reality," that is at first seen as only fictional in the usages and conventions, but later correctly seen as perhaps a more *vital* order of reality than material reality itself. Yet its unreality is sensed, too. At the conventions level, one acts as if the fictions were real up to the point of wringing from them their face-conserving considerations and adjustive utilities. But it is a tragic error to take them as actually real—to transform their face-saving into paranoid self-confidence, their coquettish deviousnesses into deathless pledges, or to inflict yourself upon the prime minister because he said he hoped to meet you again soon. At the mores and institutional levels, the counter-mores develop. These parallel (with a great divergence) the moral and spiritual values of the group. There develop the accommodative devices of forgiveness and reinstatement already mentioned. The group will crucify the one who insists upon acting as if the ideals were actually real about as readily as it will burn at the stake the one who denies their greater reality over materially-defined truth. The illusion must be kept and lived by. But it must not be disclosed that it is not actually real; or yet lived out as if it were. Mankind, thus far in history, has needed his practices and his ideals; his realities and his dreams. He has needed them both. He has needed to believe that they are the same thing; that they are different orders of reality; that now this one, and now that one is the more imperative, the seat of the greater wisdom, the depository of the higher and richer values. It is because the whole area is so full of paradoxes, so recondite and yet so compelling, so full of inmost yearning and so tyrannous in its "exteriority and constraint," that this area is so fertile in personality conflict, exploitations, social tyrannies, and bizarre and superstitious imaginings. It is our conception that the functional human-social sciences, slowly and arduously enough at first but with some of the inductive culture's characteristically cumulative and accelerative mastery, might one day ease these tensions, converge diverging lines, bridge widening gaps with smooth accommodative devices, and validate legitimate, *natural-order* "ethical" and "spiritual" meanings—involved fictions and all. Then the social order and the cultural framework might begin to yield on a global or inter-tribal scale, that inclusive, consistent, and richly meaningful configuration of the total universe and the individual's relation to it that shall resolve the age-long internal and external conflicts and produce those creative and realizational harmonies and communions which have been the common aim of humanitarian striving, art, religion, ethics, philosophy, psychiatry, and the functional sciences.

NEW MEMBERS

ELECTED MAY 16, 1944

ACTIVE MEMBERS

D'Alelio, Gaetano F., Ph.D., High Polymers, Director of Research, Pro-phy-lac-tic Brush Company, Florence, Massachusetts.

Keeffe, George C., Ch.E., M.S., Chemical Engineering, Associate Professor in Chemical Engineering, Newark College of Engineering, Newark, New Jersey.

Leathem, James Hain, Ph.D., Endocrinology, Assistant Professor of Zoology, Rutgers University, New Brunswick, New Jersey.

Loewi, Otto, M.D., Biology, Psychology, Biochemistry, Research Professor, New York University College of Medicine, New York, N. Y.

Lowenstein, Bertrand E., M.D., Endocrinology and Metabolism, Research Fellow in Medicine, New York Post-Graduate School and Hospital, New York, N. Y.

Oglesby, Nicholas E., Ph.D., Chemistry, Technical Director, Behr-Manning Corporation, Troy, New York.

Sklar, Alfred Lee, Ph.D., Molecular Structure, Assistant Professor of Chemistry, Catholic University, Washington, D. C.

Somberg, Ethel W., B.A., Biochemistry, Assistant Research Chemist, V.D. Research Laboratory, U. S. Marine Hospital, Stapleton, Staten Island, New York.

Wintersteiner, Oskar Paul, Ph.D., Organic Chemistry, Biochemistry, Head, Division of Organic Chemistry, Squibb Institute for Medical Research, New Brunswick, New Jersey.

Wolf, William, M.D., M.S., Ph.D., Endocrinology, Consulting Endocrinologist, New York University School of Dentistry and Misericordia Hospital, New York, N. Y.

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